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The Six Pips*

Twenty-one Years of Greenwich Time Signals

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The BBC now makes Greenwich time available not merely through the length and breadth of the British Isles, but also to the remotest parts of the Earth.

These time-signals were first broadcast as a result of a suggestion made by Mr. Hope-Jones, the pioneer in many developments in electrical timekeeping.

In the early days of the BBC Mr. Hope-Jones used to give a broadcast on the Saturday evenings before the beginning and ending of summer time, to instruct the public how best to put their clocks forward or back. These broadcasts were timed to end at nine o'clock and, in order to enable watches to be set correctly to the new hour, Mr. Hope-Jones used to count the last five seconds of the hour, with the aid of a watch set to the correct time, thus: fifty-five, fifty-six, fifty-seven, fifty-eight, fifty-nine, zero. This method of giving the time came to be followed by some of the announcers, but the time given was often not accurate. Mr. Hope-Jones therefore suggested that as the BBC had the means for distributing time but hadn't got the accurate time, and as the Greenwich Observatory had the accurate time but had no means of distributing it, cooperation between the Observatory and the BBC would make accurate time available. He suggested also that the signals should take the form of six short dots or pips, ending at the exact hours, because the public had become familiar, to some extent, with such a time-signal. Sir Frank Dyson, who was then Astronomer Royal, needed little persuasion. For it has always been the policy of the Observatory to make accurate time as widely available as possible. So the Greenwich time-signals, in their now familiar form, came into being.

It may be of interest to recall the successive stages by which Greenwich time has been made available. The first modest step was taken in 1833 when a time-ball, 5 feet in diameter, was erected on the top of the main building of the Observatory. This time-ball was dropped daily at one o'clock P.M., mean solar time, the first instant of its downward movement marking the hour. The purpose of this time-ball signal was to enable ships in the River Thames and in the docks nearby to regulate and rate their chronometers, which give them Greenwich time when they are at sea. Nothing more than this could be done to provide pub-

*Originally a British Broadcasting Corporation broadcast.

lic time-signals until the development of telegraphic communications. In 1852 an electric clock—or a galvanic clock, as it was called in those days—was installed at the Observatory, which sent an electric signal each day and caused a time-ball on the offices of the Electric Telegraph Co. in the Strand to drop. The following year a time-ball was installed at Deal, which was dropped at one o'clock daily, by electric signal from Greenwich, for the benefit of shipping in the Downs. Time-balls were also installed in later years at the various Admiralty dockyards.

In the middle of the 19th century, Greenwich mean time had not come into general use throughout Great Britain. Time is, of course, a local phenomenon. For every degree of longitude that we move westward, mean noon occurs four minutes later, so that there is a difference in local time between London and Corwall, for instance, of more than 20 minutes. In 1858 it was held in the case "Curtis *v* March" that the time appointed for the sitting of a court must be understood as the *local* mean time at the place where the court sits and *not* Greenwich time, unless it is so expressed. A new trial was granted to the defendant who had arrived at the local (Carlisle) time appointed by the Court to sit, only to find that the Court had met by Greenwich time and that in his absence the case had been decided against him. Greenwich Mean Time was not made the legal time of Great Britain until 1880. But the railways could not wait for the tardy process of the law to enact this reform; it was not possible, of course, to prepare railway time-tables keeping to the different local times of each place. So the railways decided to adopt Greenwich Mean Time throughout their system and in 1865 a service of hourly time-signals to provide the railways with time was begun; the signals were sent from Greenwich Observatory to the Office of the Electric and International Telegraph and from there were distributed to the railways. For this reason our grandfathers or great grandfathers always spoke of 'railway time' where we now speak of Greenwich time.

After the telegraph system was taken over by the British Post Office in 1870, the Post Office became responsible for distributing by telegraph the hourly signals supplied by the Observatory. You could, by paying an annual rent, have the hourly signals sent to your private residence or to your place of business.

But the distribution of time in these ways did not benefit a very wide public. It was the development of broadcasting and the institution of the six-pips signals that really made accurate time generally available. The result of this is that the public has become time conscious. Wherever the six-pips are heard, people can be seen looking at their watches or clocks. Probably some fifty to one hundred million watches and clocks in Great Britain alone are checked by the six pips. It used to be a common thing to see clocks in public places and on churches several minutes in error. That is rarely so nowadays, though during the Blitz

many clocks could be seen that had completely stopped or were hopelessly wrong. There is no doubt that the provision through the BBC of daily time-signals, whose accuracy can be relied upon, has been much appreciated.

A further step in making accurate time widely available was taken in 1936, when the Post Office installed the Speaking Clock. I had the privilege of making the first call on the Speaking Clock at an inaugural ceremony in the Holborn Telephone Exchange, presided over by Major Tryon, the then Postmaster General. The Speaking Clock is an ingenious apparatus, designed in the Post Office Research Laboratories, and it is kept near to correct time by automatic control by the hourly signals from the Observatory. It forms an exchange on its own. By dialling TIM (in the London area) or asking for 'Time,' the correct time can, in Great Britain, be obtained by telephone at any moment of the day or night.

The time is announced at intervals of 10 seconds, by a system of disks carrying sound-tracks. Sometime ago a competition was held to select 'the Girl with the Golden Voice' from among the Post Office telephonists for making these records. The Speaking Clock has placed Greenwich Time continuously on tap. The calls on it amount to about 20 million a year, which proves that there is a real need for the correct time. Your watch or clock may have stopped and you may need to know the time without waiting for the six-pips signal; and, of course, many British business concerns which do not possess radio receiving sets use the Speaking Clock to obtain time.

In 1927 the Observatory started the distribution by radio of a different type of time-signal, designed primarily, like the first time-ball signal of nearly a century earlier, for the use of shipping. These signals are sent out twice daily via the Rugby radio station; they extend over five minutes and are designed to enable the error of a chronometer to be accurately determined. They are now radiated on both long and short waves and have a practically world-wide range; but they cannot be heard with ordinary broadcast receivers. These signals are given the highest possible accuracy and corrections to the time at which they are actually radiated are subsequently published by the Observatory for purposes, such as surveying, which need a higher degree of accuracy than is required for normal navigation purposes.

The usefulness of the six-pips signals is not limited to giving the right time to the public. The signals do not have the very high accuracy of the special time-signals sent out via Rugby radio but they are accurate to about the one-twentieth part of a second. For the ordinary purposes of everyday life, this degree of accuracy is not needed. But there are many purposes for which accurate time is really essential, and the six-pips signals cater for these also. The astronomer in his Observatory needs accurate time for timing many of his observations; it is

simpler for him to take his time from Greenwich, instead of having to make special observations to determine it for himself, as he used to do. The scientist in his laboratory needs accurate time in many of his investigations. The surveyor in the field needs it, to determine his position. The watch and clock-maker or repairer needs it in the adjusting and timing of watches and clocks. The six-pips signals are extensively used by coastal shipping for navigational purposes. Since D-day, at the special request of Field Marshal Montgomery, the signals have been radiated at each hour of the day and night on certain services, so that artillerymen can correctly set their battery watches and so ensure the correct synchronization of gun barrages on the Western Front.

It is, of course, important that the pips should never be sent out at the wrong time and to guard against this possibility the BBC makes it a rule that any program with the pips in it may not have the pips recorded, because the program may be produced at some different time.

How are the pips sent? A regulator clock at the Observatory has been fitted with specially constructed contact work, so that the escape wheel of the clock closes an electrical circuit at each of the fifty-fifth to the sixtieth seconds of the minute at every quarter-hour and sends electrical signals, quite automatically, by land line direct to the BBC. The signals are not tapped out, as some people think, by hand. The clock which sends the signals is controlled and synchronized by one of the master pendulums, mounted in an air-tight case in a chamber maintained at a constant temperature. The error of this master pendulum is kept small and is corrected each day.

It must be emphasized that whereas it is the *first* stroke of Big Ben after the chimes that marks the hour, it is the *sixth* of the pips that marks the hour, half-hour, or quarter hour. The chimes of Big Ben warn you to be ready for the first stroke. The first five pips give you time to look at you watch in readiness for the sixth pip. The duration of each pip is about one-tenth of a second and it is the beginning of the sound of the last pip that marks the precise instant of the hour or quarter.

Quartz crystal clocks of very high precision are now being installed at the Observatory and will replace the present pendulum clocks as time standards. The six pips will shortly be controlled by one of these new clocks. This change will enable a still higher precision in the signals to be attained.

With the six-pips time-signals, twenty-one years old on February 5, and the Speaking Clock service, correct Greenwich time has now become readily available to everybody in Great Britain. It is the progress of science that has made this possible.

Twenty-five years ago time could not have been given with the present accuracy of one-twentieth of a second. This was made possible by the development, by British horologists, of pendulum clocks far sur-

passing in accuracy any clocks previously made. The first of these clocks was installed at the Greenwich Observatory in 1924. Then broadcasting, which I think is, without a doubt, one of the scientific wonders of this century, has enabled the correct time to reach those who want it. The suggestion made by Mr. Hope-Jones has proved a fruitful one and the six-pips time-signals have become a permanent and much appreciated feature of all BBC programs.

GREENWICH, ENGLAND, JUNE, 1945.

The Study of Stellar Variation

By DEAN B. McLAUGHLIN

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CHARACTERISTICS OF THE "GREAT SEQUENCE"

The variables of the "Great Sequence" exhibit several important relationships. The most extensive is the period-spectrum relation. It is not quite certain just how far individual stars of the Cepheid and Mira types deviate from it, but some departures are pretty surely real. However, the recognition of the abnormality of Cepheid spectra at maximum, contrasted with their normality at minimum, may make it necessary to revise the relation. There may be other defects in our spectral classifications. For example, the RV Tauri stars at minimum show TiO bands superimposed on spectra even earlier than K0, and similar behavior is found in SX Herculis. How many stars have been classified as early M, simply because they show TiO bands? The RV Tauri stars show a large rather random scatter from the period-spectrum relation. The semi-regular variables other than the RV Tauri class deviate mainly in the direction of too late a spectrum for the lengths of their periods. Thus, several stars of class M (for example, Z Leonis) have periods of 50 to 80 days; AF Cygni has a period of about 90 days. Are these stars pulsating in an overtone instead of the fundamental?

The period-luminosity relation of the Cepheids still presents some problems. Empirically it is now established with great exactness. At the short-period end, however, the near uniformity of luminosity of the cluster variables has long been a puzzle. Regardless of period, their absolute magnitudes are grouped around 0.0. The difficulty has been at least partially resolved by Schwarzschild,[55] from a study of the variables in the cluster Messier 3. He shows that the variables of Bailey's classes a and b follow closely the theoretical period-density relation, but the class c variables with periods less than 0.4 day have periods shorter than the relation requires. It is suggested that they are pulsating in the first overtone. Miss Pishmish[56] has further pointed out that the relation of color to magnitude of the stars of classes a and b resembles the main

sequence of the spectrum-luminosity (Russell-Hertzsprung) diagram.

The truly fundamental relation is one of period to density. Among giant stars (classical Cepheids) there is a steady trend towards lower luminosity with increasing density—hence the period-luminosity relation. There is also a steady trend towards earlier spectral class with increasing density—hence the period-spectrum relation. But near class A a turning point in the spectrum-density trend is reached, and down the main sequence the spectrum grows *later* with increasing density. The cluster variables are near the turning point, and, though they fit the period-density relation very well, they show a good deal of scatter when luminosity is plotted against period. The trend, however, is the opposite of the period-luminosity relation for the classical Cepheids.

The zero-point of the period-luminosity relation is an observational quantity of the utmost importance for the measurement of great distances, and much labor has gone into its establishment. The most recent solution by R. E. Wilson[57] reassures us that this fundamental datum is accurately known.

A few stars of Cepheid-like character have periods longer than we usually grant recognition as belonging in the Cepheid range. One such is SX Herculis. In the Lesser Magellanic Cloud two such long-period stars evidently fit the extended period-luminosity relation. Is SX Herculis such a star, and are other stars like it members of the Cepheid sequence? We should be on the lookout for more stars with periods in the range 50 to 150 days and spectra of classes G and K.

The extent of the period-luminosity relation towards still longer periods is very doubtful. The Mira-type stars appear to form a sequence in bolometric magnitude that starts at the shortest periods with a luminosity more than two magnitudes fainter than that of the Cepheids of longest period.[58] A discontinuity thus exists in the gap which is occupied by the semi-regular variables. In the mean, the RV Tauri stars seem to fall in with the short-period end of the Mira period-luminosity relation. The picture requires clarification, but we are probably safe in stating that the Mira variables do not fall upon a simple extrapolated portion of the Cepheid period-luminosity relation.

In general the semi-regular variables appear to be more than a magnitude less luminous than the Mira-type long-period variables, though there are some supergiant semi-regular stars.[59] As already noted, the spectra of these objects are far too late for their periods.

Through the "Great Sequence" the phase relations of light and velocity curves are progressive with respect to period.[60] We have mentioned above that the precise "Cepheid-like" correlation gives way, among the Cepheids of longest period, to a distinct lag of velocity minimum after light maximum.[23] This progresses, through the RV Tauri stars and SX Herculis, in which the lag is still greater, to the Mira-like correlation in which velocity minimum comes at light minimum. The signi-

ficance of this "period-lag" relation will be discussed in the section on "Pulsation Theory."

The period-amplitude relation is a conspicuous one, but it is largely illusory. Although the ranges in *visible* light of long-period variables are much greater than those of Cepheids, the true ranges of both types of stars in *total* radiation are very similar. It is only the limited range of sensitivity of our eyes that makes the ranges appear different by selecting the maximum of the Cepheid's spectrum and the most variable part of Mira's radiation, far on the short-wave side of its energy maximum. There is also an additional effect of strong absorption by TiO that increases the range of visual variation of the long-period variables.

Possibly the strangest of the correlations is that between form of light curve and period. Among the Cepheids, including the cluster variables, there are possibly five groups.[31] In each group, except the one of longest period, the stars of short period have very asymmetrical curves, and with lengthening period the curves become more symmetrical. In the fifth group, however, (period 10 to 45 days) the curves are at first symmetrical and become more and more asymmetrical with increasing period. The same tendency occurs among the Me variables, as noted in a previous section; those of shortest period have curves of Ludendorff's β-type (symmetrical), and with increasing period the α-type becomes dominant.

Novae and Nova-Like Variables

Apparently most of the physical phenomena of a nova outburst can be duplicated qualitatively without the shattering violence of the typical nova explosion. Any division between typical novae and such strange objects as Z Andromedae would be purely arbitrary. Recent episodes in the history of the true nova T Coronae Borealis 1866 are startlingly similar to the behavior of Z Andromedae, and we may legitimately inquire whether the latter star at some time in the unrecorded past actually passed through a typical nova outburst. From Z Andromedae it is only a short step to CI Cygni, AX Persei, RW Hydrae, AG Pegasi, and BF Cygni. To this list we can add R Aquarii, a typical class Me long-period variable, which has a variable nova-like unresolved companion with a high excitation emission spectrum and strong violet continuum. All the stars in the preceding list have such composite spectra, a class M giant and a peculiar bright-line spectrum.[61] In Z Andromedae, T Coronae, AG Pegasi, and BF Cygni a continuous spectrum with great violet extent is superimposed on the class M spectrum, whereas in AX Persei and CI Cygni this blue continuum is not clearly seen.

Z Andromedae spends several years at minimum, then within a few months it brightens through two or three magnitudes, fluctuates in a quasi-period of several hundred days, and gradually declines, during several years, to its pre-outburst brightness. Such outbursts have been recorded in 1901, 1914, and 1939. The last was closely followed spec-

trographically. The spectrum showed a number of stages whose appearance and place in the light cycle were close counterparts of those of a slow nova. Near maximum it had a supergiant absorption spectrum, displaced violetward as in novae, and P Cygni-type emission and absorption at the hydrogen and enhanced metallic lines. As the star faded it entered a metallic bright-line stage, followed by a nebular stage of increasing excitation. The outburst temporarily submerged the class M spectrum, but the fading revealed it again, quite unchanged. Evidently there are two stars, one a nearly constant M giant, the other a nova-like variable.

Stars of this sort give us a chance to study in somewhat leisurely manner the changing physical conditions in the ejected matter and the nuclear star of a nova. Not all the phenomena can be related simply and directly to those of a typical nova, but the resemblance is close in many respects. These stars are worth all the attention we can give them.

T Coronae Borealis[62] was a typical nova in 1866. For many years it was pointed to as an example of a post-nova star that had developed into class M. In 1938, however, a marked brightening occurred, and a strong continuous spectrum of a very hot star with bright hydrogen lines simply submerged the M-type spectrum. The latter was revealed again as the light faded. Here again, the M star is evidently constant and the blue companion, an old nova, has occasional minor outbursts.

Rather similar behavior was exhibited by R Aquarii[63] in 1929-1934, when it showed a P Cygni spectrum that temporarily obliterated the class Me spectrum of the long-period variable. This pair of variables is enmeshed in a peculiar nebula in which expansion has recently been detected. The rate of motion indicates that the gases left the star some 600 years ago (if their velocity has remained constant) and it is suggested that the hot companion of R Aquarii is a *bona fide* old nova.

Intermediate between T Coronae and stars of the Z Andromedae type, we find RS Ophiuchi,[64] the recurrent nova of 1898 and 1933. At the latter outburst it showed a spectrum much like that of a typical nova, except for the absence of definite absorption lines on the violet emission edges.[65] Instead, the whole violet half of each emission band was overspread by diffuse absorption, and a strong sharp absorption line lay near the *middle* of the emission, only slightly displaced towards the violet. Evidently there was a continuous ejection of matter with a very great spread of velocities, rather than a great burst that threw off a shell with a fairly definite velocity. Another peculiarity of RS Ophiuchi was the appearance, in later stages of its decline of light, of the coronal emissions, λλ 5303 and 6374.[66]

One other repeating nova has been observed spectrographically. T Pyxidis, during its maximum in 1920, showed a spectrum that was quite indistinguishable from a typical nova spectrum, with multiple broad absorptions on the violet edges of broad emissions. Another out-

burst of T Pyxidis has just been reported (*Harvard Announcement Cards* 700 and 701). Unfortunately it was not caught until it was well down in the decline, *but it then showed the coronal emissions!* Two recurrent novae have shown these lines; are they especially characteristic of recurrent novae in general? Possibly the requisite conditions of density, intensity of radiation, and electron temperature are best satisfied in the ejected cloud about a recurrent nova.

RS Ophiuchi, like Z Andromedae, has a companion spectrum of late type.[64] It is visible only at light minimum, and then with some difficulty. The spectrum of T Pyxidis at minimum, on the other hand, is continuous, with weak hydrogen and HeII emission, like many other typical novae in their post-eruption stage.

Three recurrent novae are positively known, with inter-eruption intervals of 12 to 43 years. No similar stars are known with intervals of the order of three or four years. Perhaps such objects do not exist; perhaps they do, and await discovery; possibly some have been found but remain to be recognized. We must keep our eyes open.

The U Geminorum Variables

From the light curves of U Geminorum stars, we could classify them as miniature novae. They have sudden eruptions at intervals of a few weeks to several months, and ranges of from two to four magnitudes. The intervals are irregular, the minima are usually quite flat, the rise to maximum very swift (one or two days only), the fading considerably slower, and the total duration of such an outburst a few weeks at most. The related Z Camelopardalis stars have shorter intervals and smaller ranges, spend much less time at minimum and may remain for some time at intermediate brightness.

The spectra of U Geminorum stars[67] have some points of similarity to those of novae. At maximum the spectra are continuous, sometimes with vague absorption lines of hydrogen and helium. As the light fades, wide emission "bands" of hydrogen and helium appear, but unlike those of nova spectra, they have no displaced absorptions on their violet edges. As the star fades, the continuous spectrum weakens more rapidly than these broadened bright lines, so that the spectrum at minimum resembles that of a Wolf-Rayet star of low excitation.

There are two marked exceptions to the above statements. The star RU Pegasi at minimum has a dwarf G absorption spectrum with superimposed bright lines. AE Aquarii[68] at minimum also has a dwarf G spectrum with emissions, but in addition it shows large variations of radial velocity, while the emissions remain fixed.

Elvey and H. W. Babcock[69] found marked changes in the continuous spectra of U Geminorum stars from minimum to maximum. At minimum the energy distribution is like that of class G, while at maximum it appears like that of classes F, A, or B, in order of increasing range

of variation. The change of brightness is thus associated with a large change of color temperature. This is just the opposite of the change observed in the continuous spectra of novae. At minimum the novae have very "hot" continuous spectra (great extent in the violet) while at maximum the energy distribution is comparable to that of class F.

Do the "cool" continuous spectra at minimum mean that class G stars are present in all cases, but that their absorption lines are so greatly broadened or otherwise masked as to be unrecognizable? In that case, is the increase of light due to a heating of the G-type star itself, or to an outburst of a fainter companion? The existence of one binary (AE Aquarii) in the class suggests the latter possibility, as does also the fact that Elvey and Babcock report the absorption lines of RU Pegasi apparently masked, while Joy's plate (which the author has seen) shows a normal well-defined class G spectrum. There is here a certain analogy with the nova-like variables which are associated with giant M stars, as in Z Andromedae, AG Pegasi, and R Aquarii. Perhaps the U Geminorum stars are dwarf counterparts of these; the similarity is too close to be dismissed as a mere coincidence.

The R Coronae Variables

Of all the types of variables that have been known from before the beginning of our fifty-year period, the R Coronae-type is the least understood. Stars of this class are rather few and, although the light changes have been closely followed, their spectra have not been extensively studied. The reason is fairly obvious; astronomers are only human, and they concentrate on problems that show the most promise of "paying off."

The constant maxima and deep irregular minima suggest occultation as the cause of variation. Dark nebulous wisps immediately come to mind. The explanation cannot be so simple, however, since R Coronae at minimum shows a change to a bright-line spectrum.[70] The alternative of engulfment in the dark nebula might explain the bright lines. But it is hard to conceive of a dark nebula of sufficient optical depth to dim the star several magnitudes, and yet of such small dimensions that the star can pass across its diameter in a few years at the most. The cause must be physical rather than geometrical.

One of the stars of this class that has been most thoroughly studied is the "iron star" XX Ophiuchi.[71] Unfortunately, it may not be typical, for its range is only about one magnitude. Its P Cygni and emission spectrum which changed to a supergiant absorption spectrum and back again is reminiscent of the variations of a nova spectrum when it fluctuates near maximum light. The suspicion arises that R Coronae stars may be extremely slow novae—far slower than RT Serpentis—which remain so long at maximum that they masquerade as supergiant stars, but which occasionally suffer relapses or interruptions of activity, so

that the expanded shell of gas, or pseudo-photosphere, is able to clear away before a new one is formed to take its place.

Such a model was suggested by the writer[72] to account for R Coronae variation, and a similar model was developed in some detail by Baldwin[73] to account for the correlated light variations and spectral changes of γ Cassiopeiae, whose complexity excludes it from description here, beyond the mere mention that the formation and clearing away of a "false photosphere" accounts satisfactorily for the variations of light, color temperature, and computed radius, as well as the formation and fading of a "shell" absorption spectrum.

The T Tauri stars,[74] recently separated from the R Coronae class, are even less known to astronomers generally, and we shall await with interest the publication of Joy's work on these objects.

Other Variables

The β Cephei variables, with small range and very short period, need the attention of photoelectric observers, in conjunction with spectroscopic observations. The multiple variability of δ Scuti[75] is perhaps but one example of the sort of surprises in store for us if we search carefully enough. Slight variations may occur in many supposedly constant stars.

The Be spectrum variables are a challenge to patient photoelectric observers. Several of these emission stars are variables of small range or are strongly suspected. It is unfortunate that there are no perfectly conclusive observations to show whether Pleione changed in brightness when it recovered its hydrogen bright lines and "shell" spectrum. Perhaps γ Cassiopeiae is only the most conspicuous example of variability of a star in connection with the development and fading of a "shell" spectrum.

The star R Monocerotis, at the tip of Hubble's variable nebula, requires attention. No account of spectral changes has been published, thought it should be an easy object for the powerful low-dispersion spectrographs of today.

Too little is known also of the rapid irregular variables of RR Tauri-type, and of the numerous faint variables in the Orion nebula.[76] There are not even enough data to permit a guess as to whether they form homogeneous or heterogeneous spectral groups.

Earlier Theories of Stellar Variation

It is hard to believe, today, that such hypotheses as the formation of great slag crusts ("Schlackenfelder") over the surface of a fluid incandescent star, and the later breaking up and fusion of those crusts, could have been seriously entertained. The laws of radiation and other knowledge that has come since the application of the spectroscope are so much a part of our consciousness that we apply them without being aware of it. But when these facts and laws were unknown, it was pos-

sible to believe in the fracturing of solid crusts, to permit underlying glowing fluid to shine through or hot gases to erupt. More recently it was possible to believe in the variation of stars as due to increased and decreased spottedness, or to rotation of a heavily spotted star. This was more difficult to refute, for spots certainly are regions of lower temperature, so that the spectra might even be used to bolster the spot hypothesis, for variables are cooler at minimum. However, radial velocities decide against such an interpretation of the Cepheids. And the ranges of variation would almost require that an entire hemisphere of the star be one huge spot!

At the beginning of our fifty-year period, Sir Norman Lockyer's collision hypothesis[77] enjoyed considerable popularity. He explained variation in terms of two meteor swarms in mutual revolution in eccentric orbits, so that at peri(astron?) (what should one call it anyway?) their outer fringes would merge and collisions of meteors would produce heating, while at ap(astron?) no collisions would occur.

The veil theory was suggested by Merrill[78] to account for long-period variation. He suggested that condensation of gases in the outer atmospheres of these stars would produce a semi-opaque veil, which would retain the photospheric heat and cause a rise of temperature. This, in turn would result in vaporization of the veil, after which the temperature would fall and the cycle would be repeated. The veil theory is not supported by spectrophotometric measures[37] if by veiling we mean entirely non-selective obscuration. However, the equivalent of veiling does occur in large regions of the spectrum, through the overlapping of bands of TiO.

The Rise of the Pulsation Theory

Certainly the most important development in the interpretation of variable stars has been the establishment of pulsation as the most probable mechanism of periodic intrinsic variation. Although pulsation was suggested as a possibility many years earlier,[79] it was not until about 1914 that it was pressed vigorously enough[80] to overcome the opposition of preconceived ideas. Up until that time the Cepheids had been explained in terms of binary motion. The most powerful argument for orbital hypotheses was the fact that the radial velocities of most Cepheids can be represented very accurately by elliptical elements. Cases that disagreed were attributed to the superposition of a secondary variation with a period which was a submultiple of the principal period.

On the orbital interpretation, the characteristic correlation of light and velocity implied brightening of the forward side of the star, and a resisting medium was postulated to account for it.[81] As more "orbits" were determined it was found that in almost every case the rapid approach came very soon after periastron; in other words, ω was near 90°. Even this improbable selective orientation of the orbits was not too disturbing, for the greater heating due to higher speed at periastron

would give such stars a greater range of light than those whose orbits were oriented otherwise, and therefore they would have a better chance of being discovered as variables.

It was possible to believe that Cepheids were binaries so long as nothing was known of their true dimensions and so long as the laws of radiation were either not known or not appreciated. But as trigonometric parallaxes and proper motion data accumulated, it became clear that they were giant stars. Their luminosities must be some hundreds of times that of the sun, and since their temperatures (according to the spectral classes) were about the same as the temperature of the sun, their surface brightness also must be similar, and therefore their radii would have to be more than ten times that of the sun. Now, with a spectrographic orbit having $a \sin i$ about equal to the sun's radius, it was clear that so large a star moving in so small an orbit could hardly be said to have a forward side! And if the low value of $a \sin i$ was blamed on a small value of $\sin i$, then the orbit would be viewed almost in plan and there would be practically no presentation effect. If, on the other hand, the star were pulsating, the "orbital" velocity would represent simply the speed with which the absorbing layer (and hence, it was usually assumed, the photospheric surface) approached and receded from the earth. Then $a \sin i$ would be simply half the total linear displacement of the surface, a mere fraction of the star's radius. These common-sense arguments eventually broke down the binary interpretation completely and, lacking any other adequate explanation, astronomers began to entertain the pulsation theory seriously.

Internal pulsation of stars was then investigated theoretically by Eddington.[82] He showed that it was adequate and physically possible; moreover, the observed periods of Cepheids agreed in order of magnitude with those predicted theoretically for bodies with the masses and dimensions of Cepheids. But the observed phase relation of light and velocity proved troublesome. The star should be hottest and brightest when it was most compressed (smallest), and coolest and faintest when it was most extended. Instead it was hottest *later,* when the velocity curve showed it to be expanding most rapidly, and coolest when it was contracting most rapidly. Tentatively it was suggested that this difference of phase indicated the time required for the heat to make its way from the stellar interior to the surface. But it soon was found that the lag would be longer than expected; to reach the surface from the internal adiabatic region, the energy would require many periods instead of a fraction of a period, and internal variations would be smoothed out on the way. Eddington[83] later tried to discover what would happen outside the adiabatic region, near enough to the surface so that the heat could get out quickly. Again an impasse was reached; no lag would occur, and the star should be hottest when it was smallest. Thus, for several years astronomers continued to accept the pulsation

theory because no better was available, but they recognized its imperfections and hoped for a resolution of the contradiction.

The New Pulsation Theory

In the meantime some very significant new effects in Cepheid velocity curves were being discovered at the University of Michigan. In 1923 J. A. Aldrich[84] found velocity differences between the hydrogen and metallic lines in the spectrum of S Sagittae, and when the chromospheric lines were isolated they showed differences of an intermediate character. The main effect was a lag of phase of the higher levels relative to the lower, that is, the majority of metallic lines reached any given phase of the curve first, the chromospheric lines next, and the hydrogen lines last. The explanation in terms of pulsation was that a *compressional wave* from beneath the photosphere passed upward into and through the atmosphere. The velocity differences were so conspicuous that, for the first time, it seemed necessary to believe in stellar atmospheres of vastly greater extent than had previously been imagined. Yet as we became accustomed to thinking of them, these dimensions appeared neither excessive nor improbable for the atmospheres of such giant stars with low surface gravity. Naturally, certain minimum dimensions were required to avoid the absurdity of the lower levels passing through the upper ones. It was rather surprising to see how much skepticism this *a priori* expected condition encountered when it was introduced as an *ad hoc* hypothesis.

Following the first detection in S Sagittae, the spectrum of η Aquilae[84] was tested for the effect, and conspicuous success was met with. Then the analysis of 20-year-old measures of W Sagittarii[85] revealed the same sort of differences. Other Cepheids were added to the Michigan program, and each gave a positive result, though the effect was least conspicuous in δ Cephei.[86] Lines were selected on the basis of their level in the solar chromosphere, since it might be assumed that they occurred in the same order in Cepheid atmospheres. Figure 1 is a set of idealized curves to show the phase relations of light, low-level and high-level velocities. The bottom curve is a difference curve obtained simply by plotting the difference of velocity of the high and low-level curves. This expresses the rate of compression or expansion, and where it crosses the horizontal axis (zero difference), the layer between the two that were differenced is either most compressed or most expanded, as noted on the diagram.

Similar differences were found by Sanford[87] in T Monocerotis, but some observers who did not analyse their curves by differences claimed that the effect did not exist. Others looked for the effect with higher dispersion. Small differences were found, but they did not appear very satisfactory. However, no extensive test with high dispersion has been carried out except for Petrie's study of RT Aurigae.[88] Three-prism

plates, owing to their limited range of wave-length, do not give as large an assortment of lines as do those of lower dispersion.

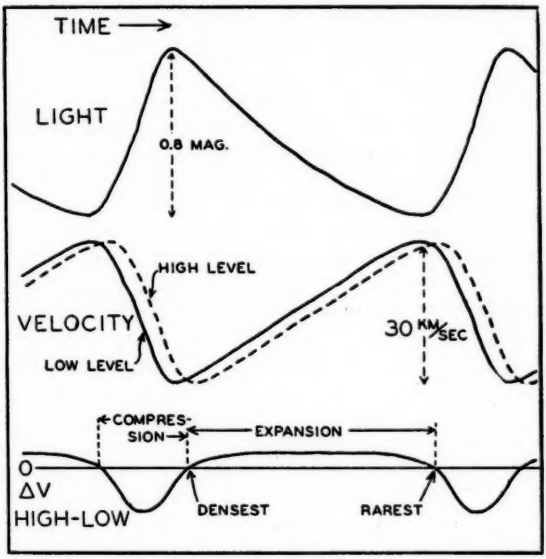

FIGURE 1

SCHEMATIC CURVES FOR A CEPHEID VARIABLE: LIGHT, VELOCITY (LOW AND HIGH LEVEL), AND ATMOSPHERIC COMPRESSION.

As more velocity curves were determined, it became evident that a "period-lag relation" existed.[23, 60] In the five-day Cepheids velocity minimum is simultaneous with light maximum. With lengthening period the velocity minimum lags after light maximum, until in SX Herculis there is fair agreement with the old pulsation theory, that is, the star is brightest when the velocity curve indicates it is smallest. The RV Tauri stars also show fair agreement. But Mira had astronomers thoroughly confused; it was hottest *too early*, for its velocity curve had its *maximum* at light maximum. In this condition matters rested for several years. Further observation seemed futile, for there were plenty of observations. What was needed most urgently was a new idea.

Eventually the new idea was forthcoming. The difficulty in the treatment by Eddington was that it was based on the concept of the whole star pulsating in phase—simply expanding and contracting, with every part moving outward and inward simultaneously. In other words, his solution involved a standing wave throughout the whole star. Schwarzschild[89] modified this by introducing the concept of an outer region of the star in which running waves progressed outward to the

surface. The deeper interior pulsated as a whole and adiabatically, and there the Eddington standing-wave mechanism was valid, but in the outer non-adiabatic region compressional waves passed outward to the photosphere and thence into the atmosphere. The observable phenomena of light and radial velocity are functions of the phase of the wave at the surface and have no direct relation to the phase of the standing wave in the deep interior (except as the time of travel determines their phase relations). But—and this is the essential point—the maximum compression of the photosphere occurs when that layer is approaching the earth most rapidly. Consider an outward push from below the surface. The impact on the next higher layer drives it upward, and that in turn drives upward the next higher. So long as the outward velocity is accelerated by pressure from below, just so long will the compression of the observed layers increase. The maximum compression and the maximum temperature will occur when the outward acceleration ceases, that is, when the outward velocity has reached a maximum.

The significance of the Michigan velocity curves of different levels now becomes clear. They revealed observationally the compressional waves traveling outward from the photospheres to the upper atmospheres of the Cepheids. At Michigan we were talking of just such compressional waves in 1923 and 1924, but so far has our science progressed that an observed fact is regarded with skepticism until a theory is found to agree with it! Now, for the first time since the overthrow of the binary interpretation, we have general accordance of theory with all the main observational phenomena of Cepheid variables. The light maximum comes as the compressional wave reaches the photosphere. From the Michigan velocity difference curves (Figure 1) we see that the maximum compression between the lowest and the intermediate layers comes just after light maximum, *i.e.*, after velocity minimum. By inference, the maximum compression of the underlying photosphere must have come just a little earlier, or just at light maximum.

For the usual Cepheid with a steeply rising light curve, the maximum and minimum of light both come when the star is decidedly smaller than its mean size. If the light maximum and minimum respectively coincide exactly with velocity minimum and maximum, and if the velocity curve has the form corresponding to $\omega = 90°$, then from the areal properties of the velocity curve it follows that the radii of the star at maximum and minimum light are the same. The total range of variation is therefore to be accounted for by changes of temperature or surface brightness alone, and changes of diameter merely modify the form of the curve.

The "period-lag relation," or increased delay of velocity minimum relative to light maximum with increasing period, finds a logical interpretation under the running wave theory of pulsation.[90] The longer the period the larger the star and the more extensive its atmosphere, *rela-*

tively to the size of the star. This means that in the stars of longer period the time of travel of the wave through the atmosphere would be not only greater, but a larger fraction of the period. The arrival of the compressional wave at the photosphere should give a *photospheric* velocity curve in which minimum velocity coincided with maximum light. But if the atmosphere is so extensive that the effective level of absorption is far above the photosphere, then the absorption velocity curve will lag, and the larger and less dense the star, the longer is its period of pulsation. Thus, in the five-day Cepheids, the effective absorbing layer is probably close to the photosphere, but with increasing period the absorption occurs higher and higher, and the velocity minimum later and later relatively to light maximum. The extreme case is Mira, in which the lag is so great that velocity minimum occurs at light *minimum.*

In the past it has been simply taken for granted that the absorption spectrum of a star is produced right down next to the photosphere. It was therefore a bold step when Scott[91] suggested that the *emission* velocity curve of Mira was the one to use for comparison with the pulsation theory. The radiometric data by Pettit and Nicholson[84] yield the temperature and bolometric magnitude of the star. From these the apparent diameter can be computed. On comparison of this calculated variation curve of the diameter with a similar curve based on the integration of the *absorption* velocity curve, it is seen that the two are hopelessly out of phase. Not so, however, if the velocity curve of the emission lines is used; for this the phases agree, and the comparison of amplitudes gives incidentally the parallax of the star.

It had been suggested long ago that the faintness of Hϵ in the spectra of long-period variables was due to absorption by overlying CaII, and later that the weakness of Hβ was due to its position in a strong TiO band. Now we have additional support for this belief, and a striking accord with the pulsation theory. The absorption layer must be very high up in the star's atmosphere, which has a depth previously undreamed of, and the compressional wave takes a good fraction of the star's period to make the journey from the emitting to the absorbing layer. Scott computes the height of the absorption above the photosphere to be about 0.5 astronomical unit. Great as this is, it is only about one-fifth the mean diameter of the star.

It will probably be some time before any detailed application of Schwarzschild's theory to RV Tauri stars is attempted. In fact we are immediately discouraged by the weakness of the emission lines of most of these variables, for there is no reliable guide to the behavior of the photosphere. With the lesson of Mira in mind, we must admit the absorption could originate almost anywhere! Let us assume, however, that the light curve itself tells us when the compressional wave arrives at the photosphere. From it we could construct a hypothetical velocity curve of the photosphere. This has been done in Figure 2. Evidently the

absorption velocity does indeed lag behind the photospheric velocity, but only a small fraction of the period. This appears reasonable; the RV Tauri stars are intermediate between Cepheids and Mira variables in period and spectrum, and it is to be expected that their atmospheres have intermediate extent and that the level of absorption will have an intermediate position. The occurrence of the bright hydrogen lines in the spectra of RV Tauri stars during the rise to maximum now makes sense, too. They would be getting strong then (due to increased excitation) and the large positive velocity of the absorption would uncover the emission lines which had already started to shift in the negative direction (if they originate close to the photosphere as in Mira). In R Scuti the emission lines are decidedly shifted towards the violet relative to the absorption spectrum, though by a smaller amount than in Mira.

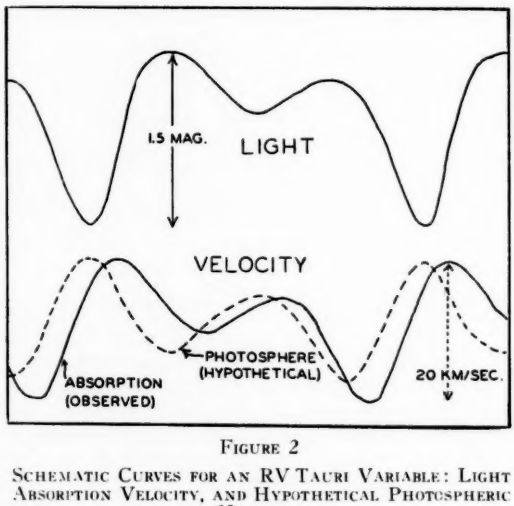

FIGURE 2

SCHEMATIC CURVES FOR AN RV TAURI VARIABLE: LIGHT ABSORPTION VELOCITY, AND HYPOTHETICAL PHOTOSPHERIC VELOCITY.

We now see a plausible reason for a part of the large negative shift of emission relative to absorption in long-period variables. At the time when the emission is strongest, it is near the minimum of its velocity curve, while the absorption velocity is near maximum. They come together late in the decline of light. There is perhaps a superimposed velocity of approach due to ejection of the emitting gas, which would prevent the curves crossing. Unless there is systematic motion of the absorbing gases outward or inward, it still appears correct to use the mean absorption velocity as the true radial velocity of the star. This is worth pursuing a little farther. In the section on "The Mira Family" we referred to the possible effect of changes of effective emitting level

upon the intensities of the emission lines. In the early part of the light cycle, the emission may be supposed to occur very deep down next to the photosphere. The line Hδ is strong because no TiO absorption occurs in its neighborhood, while Hγ and Hβ are heavily absorbed by TiO. Increasing excitation would tend to strengthen Hδ relatively, but the fading of TiO uncovers Hγ and Hβ, which therefore brighten. Now as the light declines, we might expect the reverse change to take place, but if the emitting gases have risen higher in the atmosphere (perhaps a large-scale prominence effect!) the TiO absorption would be insufficient to dim Hγ. Hβ, being in heavier absorption, would still be strongly affected. The relative velocities of emission and absorption are in accord with the supposed change of level; throughout the interval concerned, the emission lines have a considerable velocity of approach relative to the absorption.

Another well-established relation also becomes clear in the light of the running-wave pulsation theory. The increase of the violetward shift of the emission with increasing period among the Mira stars is due to the progressive increase, with period, of the phase lag between emission and absorption velocity curves.[91] The larger stars have the emitting layer farther below the absorbing layer. These are the stars of longer period; they have a greater time lag of the absorption velocity, a greater phase difference of emission and absorption, and hence a greater difference of the velocities. The fluctuation of the correlation near P = 250 days still remains unexplained. The Se stars, more highly luminous than those of class Me, and hence probably larger, show a greater emission-absorption velocity difference, which is consistent with the greater atmospheric extent that would be expected to accompany larger dimensions.

The difference of phase of the light maxima of Mira stars[34,35] and of δ Cephei[25] in different wave-lengths may now be brought into accord with the rest of the picture. Light maximum comes when the star—that is, its photosphere—is expanding rapidly, when the compressional wave is just passing through the photosphere. At this time the surface temperature is at maximum. As the wave passes on into the atmosphere, the photospheric temperature drops and the continuous spectrum weakens in all wave-lengths, but the photosphere is still expanding. Thus, there are two opposing tendencies: expansion tends to increase the total luminosity of the star through simple increase of surface area, while cooling tends to decrease its brightness. According to the laws of blackbody radiation, the effect of cooling is greatest in the shorter wave-lengths. Hence, in the shorter wave-lengths the effect of cooling gets the upper hand *before* it does so in the greater wave-lengths. This causes a delay of the phase of maximum in red light as compared with blue.

Further Problems of Pulsation Theory

The pulsation theory must eventually explain why different stars have light and velocity curves of different forms. Among the Cepheids these forms are typified by δ Cephei, ζ Geminorum, and S Sagittae, that is, unsymmetrical, symmetrical, and afflicted with a secondary maximum or a "Stillstand." Among the long-period variables there are the α-, β-, and γ-curves. These must have a physical meaning, but it eludes our grasp in the Cepheids. As for the long-period variables, we do not even have enough of the raw observational data to permit us to make a start.

Granting that the stars of the Great Sequence are pulsating, we may ask two closely related questions: (1) how did the pulsation get started; (2) how is it maintained? If pulsations were simply caused by a disturbance, such as a tidal encounter, they would very quickly die out unless some mechanism operated to keep them going. But if there are at work within the stars processes that tend to maintain a pulsation, then a very slight disturbance would set the mechanism going and the oscillation would build up to a maximum.

Years ago, before the nuclear transformations of atoms were known, Eddington suggested a subatomic process which would liberate more energy at higher temperature, and he showed that the rate of liberation must increase nearly as the square of the temperature.[92] The difficulty is not so much how to maintain a pulsation as how it can be that any giant stars do not pulsate!

The nuclear processes, of which Bethe's carbon cycle is the most familiar, are sensitive to temperature and are adequate to maintain the pulsations as long as the active material lasts. Payne-Gaposchkin and Gaposchkin,[93] making use of calculations by Greenfield,[94] have pointed out that the long-period variables and the semi-regular M and N stars derive their energy from the lithium reactions, while the Cepheids depend on the beryllium and boron processes. The RV Tauri stars appear to represent the conflict between lithium and beryllium reactions. The field is new and we can expect a good deal to happen!

Causes of Cataclysmic Variation

Origin of novae and nova-like variation in collision or tidal encounter can be safely discarded. The typical novae differ only in range of variation and frequency of eruption from the recurrent novae and U Geminorum stars. They differ in range, violence, and frequency of eruption from Z Andromedae and related stars, but the phenomena of all these objects are very similar. The facts force us to conclude that whatever the cause and however ignorant of it we may be, it is surely within the stars and does not operate from without.

Novae at minimum are very dense and very hot stars; not extreme white dwarfs, but certainly underluminous and therefore very small and superdense.[95] The Z Andromedae objects seem to be a little more

brilliant, but they are certainly far below the Wolf-Rayet stars in luminosity and are probably much denser than main sequence stars. The U Geminorum stars are very dwarfish. Evidently only such dense bodies are capable of nova-like explosions. Biermann's[96] hypothesis of an unstable layer beneath the surface appears now to be the most hopeful approach to an explanation.

In an earlier section mention was made of a suggested explanation of R Coronae variation as simply the temporary stoppage of a very long-continued maximum of a remarkably slow nova-like star. Constancy of maximum for several years was shown by the ultra-slow nova RT Serpentis.

Contributions to Galactic Studies

Variable stars show remarkable differences in their distribution and motions relative to the plane of the galaxy and to the sun. The significance of the differences is not clear. Among the long-period variables there is a very rapid change from high to low residual radial velocities with increasing period or advancing spectral class.[97] High residual velocities also characterize the cluster variables. The intermediate classical Cepheids, on the other hand, have very low residual velocities.

Systematic differences of distribution go with these effects. The cluster variables and, to a lesser extent, the long-period variables, scatter through all galactic latitudes, while the classical Cepheids have a strong galactic concentration. That the motions and distribution are correlated is logical; higher velocity would give a star greater chance of escaping far from the plane of the system. But the discontinuous character of the relations is baffling. The Cepheids, in the middle as regards period and spectrum, represent an extreme as regards distribution and motion. We cannot escape the suspicion that we have two or more mingled groups of stars that originated quite separately in space.

In this connection we note the occurrence of long-period and cluster variables in the galactic star clouds of the Sagittarius region, while classical Cepheids are rare there. Related to this is the scarcity, in our galaxy, of Cepheids with periods around two days while a considerable number of such objects are known in the Small Magellanic Cloud.

Future discoveries of variables in the galactic center will aid greatly in sounding the depths of those regions. Mere inspection of a plot of known variables shows how numerous they are in certain rectangular areas where they have been found by careful search. The vastly greater areas in which they are sparse, for no obvious reason other than that the search has not been made—or at least not yet announced—is convincing evidence that thousands of them still await discovery. So far we have only nibbled around the edges of the galactic nucleus. The cosecant law of absorption should be valid down to a latitude of 5° or so, if we avoid evident dark wisps. Relative luminosities of the different classes of variables should eventually be determinable in those regions.

The 200-inch reflector should reveal many more variables in the Andromeda spiral and M33. Possibly the cluster variables, and certainly the long-period variables (observed with red-sensitive plates) should be within range of its increased light-gathering power. NGC 6822 should yield a number of cluster and long-period variables if any such exist there.

The southern hemisphere is a rich field for variable star study. The Magellanic Clouds and the Milky Way have already yielded a multitude of variables. In the Clouds, the cluster variables are just beyond the range of the searches conducted to date. It will be interesting to see how many can be found there with more powerful equipment.

The search for variables in fields of high galactic latitude is bringing to light a number of stars previously unsuspected. These will outline pretty well the limits of the "corona" of our galaxy, and they may tell us more about the meaning of the anomalies of distribution of the better known stars. Radial velocities of selected objects among these distant high-latitude variables will be of the greatest interest.

Conclusion

The future of variable star astronomy is just as unpredictable now as it would have been 50 years ago. Great numbers of variables will surely be discovered, but the emphasis will probably be on the use of these objects as tools. For the understanding of variables as such, we already have abundant material on which to work, except for possible examples of types that are rare or unknown among our moderately close neighbors. The abundance of two-day Cepheids in the Small Magellanic Cloud is a warning against the acceptance of local statistics as universally valid. But for the understanding of the principal types we need no more new individuals, save for novae and the relatively scarce R Coronae stars and a few others.

A great deal of variable star observation will still be regarded as so much drudgery, but what, for that matter, is most really careful and accurate scientific work? The intensive following of many of the brighter variables will still be the work of the amateur who does not have elaborate equipment. The programs of the American Association of Variable Star Observers and other similar organizations have paid off handsomely and will continue to do so.

The widest field for investigation of variable stars themselves is that of theory and interpretation. The terms are used in their very broadest sense, to include the whole field of new ideas and "hunches" that will suggest new lines of observational attack or new tricks to be done with old observations.

As of 1945 we have a complete explanation of only one type of variable star, the eclipsing binary, and it has been pointedly remarked that they are not variable. Some modification of this view is now necessary,

since some eclipsing stars do present anomalies that require physical as well as geometrical explanation.

We are not too badly off as regards theories of intrinsic recurrent variation. Within the past few years the pulsation theory has been placed on a firm foundation. We now have a model whose approximate geometry and whose external physics are reasonably clear and accordant with the principal observations of the Cepheids. The Mira stars are still somewhat less known; we are just recovering from the shock of having the emission region crammed down next to the photosphere, and we still wonder what is between the emitting and absorbing regions! The origin of the bright lines remains a problem, but for that matter so is the origin of the coronal emissions in the solar envelope. The two problems are doubtless related; in both cases the lines represent excitation that is "impossible" under what we have thought are the conditions of temperature. The various forms of light curves require physical explanation.

The geometry and the external physics of the novae and related stars are well known in their broader features, though many details remain puzzling. We have little more than conjectures as to the cause of the eruptions, but we may be sure that it is internal.

Conviction is strong that the semi-regular and irregular red variables will be explained in terms of pulsation simply more complicated than in the Cepheids, but basically the same. But of the R Coronae and T Tauri variables we are still relatively ignorant, and even the raw observational data are meager and discontinuous.

On the whole, the review of what has been accomplished in fifty years of study of stellar variation is encouraging. There are many unsolved problems, but the Great Sequence is now correlated throughout by a theory which, though not complete or final, has reached a stage such that we no longer need apologize for its shortcomings.

References

[55] M. Schwarzschild, *Harv. Circ.*, No. 437, 1940.
[56] Miss Pishmish, *Ap. J.*, **101**, 204, 1945.
[57] R. E. Wilson, *Ap. J.*, **89**, 218, 1939.
[58] Payne-Gaposchkin and Gaposchkin, *Proc. Nat. Acad. Sci.*, **28**, 490, 1942.
[59] Joy, *Ap. J.*, **96**, 344, 1942; R. E. Wilson, *Ap. J.*, **96**, 371, 1942.
[60] McLaughlin, *A. J.*, **40**, 16, 1929; *Publ. Am. Ast. Soc.*, **9**, 117, 1941.
[61] Merrill, *Ap. J.*, **77**, 44, 1933; Swings and Struve, *Ap. J.*, **91**, 600, 1940; **96**, 254, 1942.
[62] Hachenberg and Wellman, *Zs. f. Aph.* **17**, 246, 1939; **19**, 16, 1940.
[63] Merrill, *Ap. J.*, **81**, 312, 1935.
[64] Swings and Struve, *Ap. J.*, **94**, 297, 1941.
[65] Adams and Joy, *Publ. A. S. P.*, **45**, 249, 1933.
[66] Adams and Joy, *Publ. A. S. P.*, **45**, 301, 1933.
[67] Joy, *Publ. A. S. P.*, **52**, 324, 1940.
[68] Joy, *Publ. A. S. P.*, **55**, 283, 1943.
[69] Elvey and H. W. Babcock, *Ap. J.*, **97**, 412, 1943.
[70] Joy and Humason, *Publ. A. S. P.*, **35**, 325, 1923.
[71] Merrill, *Ap. J.*, **75**, 133, 1932.
[72] McLaughlin, *Popular Astronomy*, **46**, 372, 1939.
[73] Baldwin, *Ap. J.*, **89**, 279, 1939; **92**, 82, 1940.

[74] Joy, *Publ. A. S. P.*, **54**, 17, 1942.
[75] Fath, *Lick Bull.*, **19**, 77, 1940.
[76] Payne-Gaposchkin and Gaposchkin, *Harv. Monogr.*, No. 5, pp. 301 ff., 1938.
[77] C. A. Young, "General Astronomy," p. 484, 1891.
[78] Merrill, *Mich. Obs. Publ.*, **2**, 70, 1916.
[79] Moulton, *Ap. J.*, **29**, 257, 1909. This paper discusses earlier work by Ritter and Emden.
[80] Shapley, *Ap. J.*, **40**, 448, 1914; Plummer, *M. N.*, **75**, 573, 1915.
[81] Duncan, *Lick Bull.*, **5**, 82, 1909.
[82] Eddington, *M. N.*, **79**, 2, 1918; 177, 1919.
[83] Eddington, *M. N.*, **87**, 539, 1927.
[84] Rufus, *Proc. Nat. Acad. Sci.*, **10**, 264, 1924; *Mich. Obs. Publ.*, **4**, 101, 1932.
[85] R. H. Curtiss, *Publ. A. S. P.*, **38**, 148, 1926.
[86] R. M. Petrie, *Mich. Obs. Publ.*, **6**, 37, 1934.
[87] Sanford, *Ap. J.*, **66**, 170, 1927.
[88] R. M. Petrie, *Mich. Obs. Publ.*, **5**, 9, 1932.
[89] M. Schwarzschild, *Harv. Circ.*, Nos. 429 and 431, 1938.
[90] McLaughlin, *Science*, **101**, 535, 1945.
[91] R. M. Scott, *Ap. J.*, **95**, 58, 1942.
[92] Eddington, "The International Constitution of the Stars," p. 201, 1926.
[93] Payne-Gaposchkin and Gaposchkin, *Proc. Amer. Phil. Soc.*, **86**, No. 3, 1943.
[94] Greenfield, *Phys. Rev.*, **60**, 175, 1944.
[95] McLaughlin, *Popular Astronomy*, **49**, 292, 1941.
[96] Biermann, *Zs. f. Aph.*, **18**, 344, 1939.
[97] R. E. Wilson and P. W. Merrill, *Ap. J.*, **95**, 248, 1942.

CAMBRIDGE, MASSACHUSETTS, MAY, 1945.
(Absent on leave from the University of Michigan.)

Meteorites and the "Carolina Bays"

By JOHN C. McCAMPBELL

Studies recently conducted in connection with the problem of the origin of the "Carolina Bays" of the Atlantic Coastal Plain of the South Eastern United States (Fig. 1) have brought to light some extremely interesting facts.

Although the bays were recognized as such by Tuomey[1] almost 100 years ago, only the period of the last fifteen years has brought any concerted effort to provide an explanation for their origin. From the findings of many workers involved, there has come a wide variety of hypotheses. However, most of these belong to one or the other of two general foundations: one, the hypothesis of terrestrial origin, advocated and so well summed up by Johnson,[2] and the other the meteorite hypothesis proposed by Melton and Schriever.[3] The meteorite hypothesis was later modified and expanded by MacCarthy.[4]

Melton and Schriever suggested that the bays are the direct result of the impact of a great many meteorites which came to the earth as a "swarm" and fell upon the surface at some time in the past. The rims surrounding the bays as well as the depressions themselves were considered as having been formed by the plowing action of the meteorites themselves. Prouty[5], and MacCarthy[4] suggested that a cushion of highly compressed air must accompany these rapidly moving meteorites and consequently would provide a tool sufficiently large and strong to bring

the bays immediately into being. It was the air cushion that was most effective in the formation of the bays rather than the meteorite itself.

As a test of the meteorite hypothesis a great many reconnaissance and detailed magnetometer surveys of typical bay areas have been carried out, the assumption being that, if a meteorite or a portion of a meteorite was buried beneath the surface of the earth, its presence would be indicated at the surface by local disturbances in the magnetic field. True to expectations, magnetic highs or disturbances were encountered in close association with the bays examined. Prouty[5] in describing such a

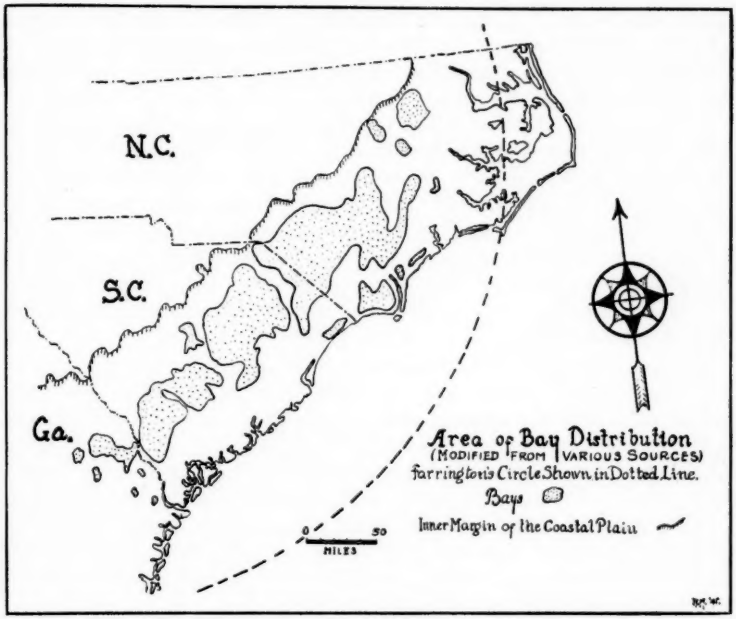

FIGURE 1

survey declared that the data collected indicated that the highs were not only located in association with the bays but there also existed a definite relationship between the highs and the bays. He devised the rule, which became known to Johnson[2] as "Prouty's rule," by which the position of the highs in association with any bay could be predicted as occurring outside of the bay and at a position somewhat south and west of the southeastern extension of the bay and at a distance equal to the short diameter of the bay. Johnson pointed out that this rule seemed in error and presented as proof two bays located outside of Turbeville, S. C., that Prouty surveyed. The present author[6] cites the results of his more extensive survey of some Bladen County, North Carolina, bays. The

data collected here show that highs are encountered. Yet, there is considerable variation in the location of the spot of the high. They occur less well coordinated with regard to the bays they accompany. This would indicate that although Prouty's rule is, in general, valid, it cannot be used to predict the exact place where the high will occur.

The surrounding sand rims which are almost always present are next to gain our attention. In a great many instances they occur as overlapping or intersecting rims. To explain this phenomenon of overlap we must resort to the meteoritic hypothesis. If we think of the meteorite swarm that fell upon the earth as having consisted of a great many objects, and probably several meteorites thick, sufficient time would have elapsed between the impact of the first and the second meteorites which landed in closely adjacent places. Thus, the crater and the rim formed by the first would be somewhat covered over and partly obscured by the rim of the second bay. None of the geological processes which have been suggested previously would provide such an elapse of time to permit the formation of the two rims. Only the meteorite swarm presents the opportunity for this phenomenon to take place so perfectly.

Johnson points out that there is some variation in the orientation of the axes of the bays in the northern portion of the bay area and those in the southern portion. The bays toward the North are oriented S50°E while those towards the South have an orientation of S20°E. The average being S45°E.

The present author once considered the hypothesis that the meteorites were scattered, in the air, by the force of the gases produced around the closely assembled, burning, meteorites. Thus those to the north and south were deflected from the straight-line path of N45°E. The front and back ones of the swarm were simply moved forward or retarded, respectively. Further consideration shows that objects such as these would have had too much kinetic energy to have been shifted materially from their straight line path which they travelled at a high rate of speed. Sufficient force could not conceivably have come from the simple burning of the meteorites.

Better still would be a hypothesis of explosion of an extremely large object (or possibly objects) at or soon after penetration of the atmosphere. Such an explosion would provide forces acting in the proper direction and would permit of the necessary scattering to provide a reason for the divergent axes of bays. This would provide a uniform scattering and a uniform variation in orientation.

Olivier suggests, in a personal communication, that the very probable irregularity in the shape of the objects themselves was sufficient to cause them to follow divergent lines of travel on their flight to the earth once they had entered our atmosphere and thus produce craters with varying directions of orientation. Unfortunately, this does not necessarily offer an explanation for the more or less uniform change in orientation from

north to south, previously mentioned.

Yet, in spite of the criticism directed at the meteorite hypothesis of origin, there seems none other that provides such logical explanations for the numerous other facts at hand.

Swarms or trains of meteorites that would be necessary for the formation of the bays are not unknown. The possibility of the impact of such a train along the Coastal Plain of the Carolinas and Georgia is certainly not preposterous. Too, it must be remembered that the area of the bays lies within the area of Farrington's Circle,[7] a fact that may or may not have more than limited importance.

The absence of meteoritic materials from the area of the bays has been used as an argument against this mode of origin. However, if the bays were formed by meteoritic impact as it has been outlined here, the absence of such materials would be the expected thing. The meteorites certainly would come to the earth in an envelope of compressed air. Such an envelope would be effective in pushing aside and protecting the sands of the Coastal Plain from contact with the meteorite itself. As the meteorite buried itself into the deeper sands, the surface materials which had been blown into the air returned to the surface and came to rest in essentially the same area from which they were removed. Thus, we see only the area of disturbed sands, not the meteorites or particles of the meteorites themselves.

Résumé

We find that field and magnetic data strongly indicate a meteoritic mode of origin of the bays.

Because of the apparent variations in the location of the magnetic high associated with the bays, it is felt that no definite rule can be devised whereby the specific locality of the high may be predicted previous to magnetic surveying.

Double rims are indicative of a lapse of time, measured in seconds or at most a few minutes, from the formation of one bay and the formation of one immediately adjacent where the rims overlap. Such time increments are presented when the meteorite hypothesis is accepted.

Variations in orientation of axes are explainable by conceiving of an explosion of the meteorite before the atmosphere was complete traversed or by the effect that would probably be brought about by irregularities in shape.

No meteoritic materials are found at the surface, the meteorite not coming in contact with the earth until it came to rest. This protection was brought about by an envelope of air surrounding the meteorite and acting with it to form the bays.

References

[1] Tuomey, Michael, "Report on the Geology of South Carolina." *Geol. Surv. of S. C.*, Columbia, S. C., 1845.

[2] Johnson, Douglas, "Origin of the Carolina Bays." New York, Columbia Univ. Press, 1942.

[3] Melton, F. A. and Schriever, William; "The Carolina Bays—Are They Meteorite Scars?" *Jour. Geol.*, Vol. 41, pp. 52-66, 1933.

[4] MacCarthy, G. R., "The Carolina Bays." *Bull. G. S. A.*, Vol. 48, pp. 1211-1226, 1937.

[5] Prouty, W. F., "Carolina Bays and Elliptical Lake Basins." *Jour. Geol.*, Vol. 43, pp. 200-207, 1935.

[6] McCampbell, John C., "A Geomagnetic Survey of Some Bladen County, N. C., 'Carolina Bays'." *Jour. Geol.*, Vol. 53, No. 1, pp. 66-67, 1945.

[7] Farrington, O. C., "The Geographical Distribution of Meteorites." *Pop. Sc. Mo.*, Vol. 64, pp. 357-364, 1904.

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The Restoration of Pulkovo Observatory

By LYDIA BACH

The USSR Academy of Sciences' main observatory is situated on the heights at Pulkovo 14 kilometres to the south of Leningrad, that is to say, the observatory was and will be situated at that spot. After two and a half years of constant bombardment from the air and from German artillery little remains of the Central building and the tower, that were built in 1839 by Briulov, or of the numerous pavilions that stood in a well-wooded park. The park itself has disappeared completely and with it disappeared the village of Pulkovo that was situated near the observatory.

The main building of the observatory was destroyed by several direct hits from heavy aerial bombs. The deep vaults of the building where the sidereal clocks were kept were laid bare by the explosions. The massive columns of the main portal were destroyed and even the heavy foundation which supported the meridian circle was badly damaged and huge pieces of it were knocked off. Only the skeleton of the tower which housed the great refractor remains. Not a trace remains of the pavilion which housed the great solar telescope.

The more valuable instruments were evacuated to Tashkent in good time where the observatory workers continued their observations. Only a small part of the library of 100,000 books on astronomy, geodesy, and allied subjects could be saved. There were books by astronomers of all periods from the earliest days of the science. Most of the books were taken into the clock vaults and suffered from a direct hit from a heavy shell. The pamphlet section and the section of atlases and maps were almost completely destroyed.

On October 24, 1944, the question of the rebuilding of Pulkovo Observatory and the further direction of its work were discussed at a special session of the presidium of the Academy of Sciences of the USSR.

Professor Grigory Neuimin, acting director of the observatory, said that Pulkovo Observatory would have to be rebuilt on its old site as old and new series of stellar observations must form one continuous

whole which would only be possible if the instruments were re-erected in the old places. He gave details of the plan of restoration which was approved by the presidium.

It was decided to rebuild the main building of the observatory in accordance with the original plans drawn up by Briulov as these are still in existence. The wings of the building will be rebuilt in accordance with the old plans but they will have other uses; they will contain a museum, a library, and a conference hall. A number of separate pavilions equipped with modern instruments will be provided for stellar observations. The tower to house the great refractor will be rebuilt on its old foundation. The vertical circle, meridian circle, and other of the bigger instruments will be housed in new pavilions. The building for the time service is also being replanned.

The whole complex of observatory buildings, designed on the lines of classical early eighteenth century manor houses, will all be within the park that will be formed by the replanting of trees and shrubs on Pulkovo heights. Dwelling houses for the astronomers and other members of the staff and their families will be built on the eastern slopes of the heights. A wide belt around the observatory will be a "preserve" on which it will be forbidden to erect any buildings; this will insure the clearest possible sky for astronomical observations.

At the same time as the rebuilding of the observatory was decided on, the presidium of the Academy of Sciences also laid down a plan for the observatory's future scientific work. The main work of the observatory will consist of problems of astrometry, the determination of the positions of heavenly bodies and their sizes, work in which Pulkovo has always played a leading role. The northerly position of the observatory gives it certain advantages in determining certain astronomical constants. The All-union Time Service and Latitude Service will again be centered at Pulkovo.

Experience gained at Pulkovo in astrophysics, the study of the chemical composition and physical properties of the heavenly bodies, is greatly hampered by the short nights in summer and the nearness of a big city with its artificially lit night sky which limit the employment of astrophysical instruments. The presidium of the Academy of Sciences, therefore, decided that the bigger and more urgent astrophysical problems will be dealt with by the observatories in the south as their solution depends on long series of observations with instruments. Methodological and laboratory research in astrophysics will be conducted at Pulkovo as will those astrophysical observations which have to be made in high latitudes. A sun service will be organized by the astrophysical department.

Pulkovo is not only a century-old center of Russian astronomy; it is also a place of historical importance to the Soviet Union. It was here that the Red Army defeated the troops of the White-guard general,

Judenich, who attacked the young Soviet republic in 1917. Here again, a quarter of a century later, the Red Army dealt the German invaders a heavy defeat. The Leningrad City soviet has decided to erect a monument to the defenders of Leningrad on the Pulkovo hills. It will take the form of a column of granite with a monumental staircase architecturally connected with the reborn observatory.

Moscow, U.S.S.R., January, 1945.

Shadow Bands at the Eclipse of July 9, 1945

By RICHARD L. FELDMAN

Dr. B. W. Currie of the physics department of the University of Saskatchewan has sent the negative report of a retired British physicist, Mr. White, who went from Saskatoon to Grenfell to observe "and was rewarded by an unusually striking display at totality. He watched for shadow bands on the side of a white house right up to the instant of totality and could detect none. The atmosphere was exceptionally (clear) due to the influx of polar air at the rear of a disturbance and he expressed the opinion that the absence of bands was due partly to the early morning hour and partly to the low turbidity of the atmosphere." The last sentence quoted discloses Mr. White as an adherent to a refraction theory.

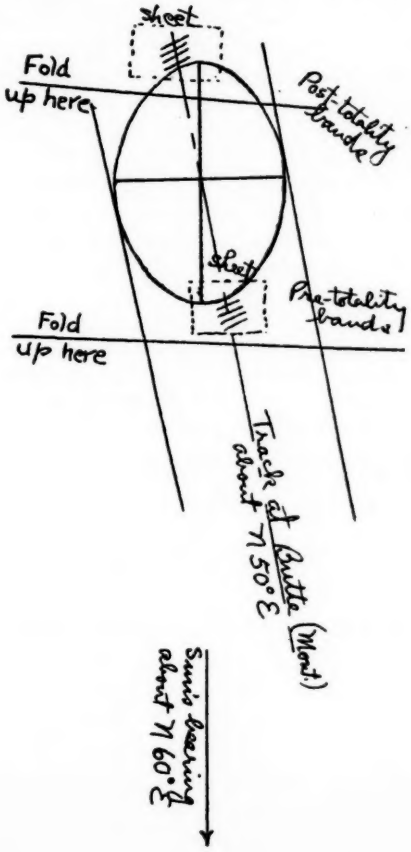
The following report from Dr. Hill seems worthwhile publishing in its entirety.

> It so happened that several of us from Montana State College were able to go over to Butte where we had a fine opportunity to witness the eclipse of July 9 in totality. As a part of our observation, we tried to obtain some information on shadow bands as you suggested.
>
> Dr. John McKinsie, of the mathematics department, was assigned this particular job. Unfortunately I was fully occupied with other readings, and was the only physicist on the campus, so that no one familiar with interference phenomena was able to make this observation. Dr. McKinsie was handicapped further in that he was not able to find a stop watch at the last moment. He was supposed to have had one, but there was some slip-up so that he had none just when he should have had.
>
> However, he did make some observations. A white sheet about four feet in height and six feet long was hung over the side of our truck, in a plane practically perpendicular to the rays of the sun. Shadow bands were distinct for a period of nearly thirty seconds immediately prior to and immediately following totality. During totality no bands were observed. These bands were apparently from four to six inches wide (time did not permit accurate measurement) not black, but nevertheless distinct. They appeared to be practically straight, and extended at an angle roughly 45° with the horizontal from the upper right (North side) to the lower left of the screen. They moved across the screen rapidly and, while they were not timed, it was estimated from Dr. McKinsie's observations and from comments made by others who saw them that they were moving at a rate of from 15 to 40 feet per second. My own feeling is that a rate of 20-30 feet per second would be about right. Definitely, they were not moving as fast as the moon's shadow.

Both before and after totality the bands moved in the same direction. Direction of progress was from the upper left (South side of screen) to lower right, again at about 45° with horizontal. As nearly as could be observed, the bands seemed to be uniform in width with spaces about the same width as the bands, and speed of movement was uniform during the time they were observed.

Another party composed of students and one instructor, climbed Mt. Sacajawea near Bozeman here. They were on the east slope, near some large snow fields, and were instructed to watch for bands. Although the eclipse at this point was about 99 per cent total, no one observed any. The other students were flying airplanes, and were in the path of totality at the time. One of these was instructed to watch for bands. He set up a card about 3 feet square on the W. side of his cabin, allowing sunlight to hit it with the window open. He saw nothing of the shadow bands.

I hope that these incomplete observations may be of some use to you. We are sorry that we slipped up here and there, and that our data are not more complete. Had I been able to make these observations myself,



I feel that I might have added a little, as could any physicist who has had experience with interference fringes. From all that was said, these seemed to have the appearance of such. These phenomena interest me a great deal, and I hope that another time I shall have the opportunity of observing them. If you publish any compilation of the information you receive on these shadow bands, I shall be glad to know about it so that I may learn more of this interesting phenomenon.

ARMIN J. HILL.

Montana State College, July 19, 1945.

My four papers on bands carried by POPULAR ASTRONOMY* dispose of several refraction theories. In reverting to the early *diffraction* explanation with my own additions thereto, I invite attention to the accompanying diagram.

There is only one place on the track where diffraction bands could be the same after as before totality, and this is on the center-line. This is also the only site where diffraction bands could extend in the direction reported, that is, tangent to the edge of the moon. Butte, Montana, where the observation was made, was in this location.

As for the possibilities of direction of progress, according to reports from other eclipses the bands might have progressed in the opposite direction before and after, or one display might have been in the reverse direction to the other in either of the two possible modes, or they might have been stationary, or one stationary and one moving.

From the rather meagre data I have been able to collect in the past, I am wondering if this Butte display is not the standard center-line phenomenon for the first portion of any eclipse track. I am not prepared to conclude that definitely, however.

While the failure of the two groups at Montana State College to see bands at higher elevations would seem to check with Mr. White's polar air explanation and the refraction theories, yet the work of Rotch and my own investigation indicate that bands are more prominent and more distinct—sharper—at higher altitudes. (Balloons were used in Spain in 1905.) The bands at Butte were "not black, but (were) nevertheless distinct." These others may have overlooked them.

BOX 265, ROUTE 2, FALLS CHURCH, VA., 1 AUGUST 1945.

*April, 1938; January, April, August, 1940.

The Planets in November, 1945

NOTE: Greenwich Civil Time is employed unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours, Central Time, 6 hours, etc. The phenomena are described as they are to be seen from latitude 45° N. The data are taken chiefly from the *American Ephemeris and Nautical Almanac*.

Sun. The sun will move southeastwardly from Libra into Ophiuchus. At the end of the period it will be near its farthest point south, marking the time of the winter solstice.

Moon. The phases of the moon will occur as follows:

	d	h
New Moon	Nov. 4	23
First Quarter	13	00
Full Moon	19	15
Last Quarter	26	13

Mercury. Mercury will be east of the sun throughout the month. It will move eastward more rapidly than the sun until November 17, at which time it reaches a point of greatest eastern elongation. It will be an evening star, and should be easily visible after sunset on and near the date of eastern elongation. At this time it will be near its point of greatest declination south, which is an unfavorable feature for northern observers.

Venus. Venus will remain west of the sun during November. It will gradually move nearer to the sun. As a morning star it will be on the eastern horizon a little more than one hour before sunrise. At the end of the month it will be only a few degrees north of the sun.

Mars. Mars will continue to move eastward more slowly than the sun. It will rise before midnight and will be quite far north. It will be in Cancer, a short distance west of the Beehive cluster.

Jupiter. Jupiter will be a brilliant morning star during this month, rising a little more than three hours before the sun at the close. It will be in Virgo, a short distance northwest of Spica.

Saturn. Saturn, like Mars, will be well situated for observation during the late hours of the night. These two planets will be quite close together in the sky. They will both be of stellar magnitude about 0.0.

Uranus. Uranus will be moving slowly westward (retrograde) in Taurus. It will be on the meridian in the early morning.

Neptune. Neptune will be above the eastern horizon a few hours before sunrise. It will, however, not be in very favorable position as yet.

Occultation Predictions for November, 1945

(Taken from the *American Ephemeris*)

Date 1945	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION *a*	IMMERSION *b*	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION *a*	EMERSION *b*	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′										
Nov. 12	ε Capr	4 7	22 6.2	−2.9	−1.2	132	22 43.5	−0.4	+2.7	186
13	κ Capr	4.8	1 45.0	−3.0	−3.9	131	2 13.5	+1.1	+3.2	177
15	336 B.Aqar	6.5	1 49.5	+0.8	+3.7	352	2 20.3	−3.2	−3.2	303
21	o Taur	4.8	6 31.0	−2.1	−2.0	124	17 25.9	−1.7	+2.3	215
21	BD+22°925	6.5	8 21.6	−1.7	+2.3	30	9 4.3	−0.9	−3.6	318
22	36 B.Gemi	6.0	3 8.9	−0.6	+1.6	72	4 14.7	−1.2	+1.2	266
23	58 Gemi	6.0	1 16.4	0.0	+0.5	121	1 59.7	+0.4	+2.0	231
23	187 B.Gemi	6.2	9 46.9	−1.6	−1.1	102	11 2.2	−1.2	−1.4	281
24	Mars	−0.2	1 39.2	+0.4	+1.0	93	2 32.0	+0.2	+1.1	271
26	42 Leon	6.1	6 7.1	−0.4	+2.6	65	6 58.9	−1.1	−1.2	329

OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′

Nov.	8	7 Sgtr	5.5	23	57.7	−1.8	−2.2	126	0	57.2	−0.3	−0.2	230
	9	9 Sgtr	5.9	0	52.6	..	..	164	1	9.1	..	..	190
	11	308 B.Sgtr	6.3	1	48.0	−2.4	−3.1	132	2	25.6	+0.6	+1.7	196
	12	ε Capr	4.7	21	27.9	−1.5	0.0	127	22	16.5	−1.5	+2.5	201
	13	κ Capr	4.8	1	2.8	−2.4	−0.6	97	2	9.7	−0.7	+0.9	211
	21	o Taur	4.8	5	52.4	−2.1	−0.4	114	6	51.6	−1.3	+2.7	215
	21	BD+22°925	6.5	7	44.7	−1.6	+2.3	40	8	45.1	−1.9	−2.0	299
	21	175 H'Taur	6.5	13	13.6	−0.9	+0.7	36	13	45.9	+0.7	−2.7	324
	22	36 B.Gemi	6.0	3	0.7	+0.1	+1.8	56	3	54.5	−0.7	+0.9	282
	22	BD+23°1491	6.5	13	13.3	−0.4	−1.5	104	14	11.7	−0.2	−1.2	270
	23	187 B.Gemi	6.2	9	16.8	−2.0	−1.4	122	10	32.4	−2.1	+0.3	254
	26	42 Leon	6.1	6	0.0	+0.3	+2.3	61	6	45.2	−0.7	−0.8	327

OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′

Nov.	1	ζ Virg	5.1	13	19.3	−1.0	+2.6	69	14	7.6	−0.6	−2.6	348
	8	7 Sgtr	5.5	23	0.8	−2.3	−1.0	122	0	24.8	−2.0	−0.1	248
	8	9 Sgtr	5.9	23	53.1	−2.4	−1.9	137	0	58.3	−1.5	+0.5	228
	11	308 B.Sgtr	6.3	0	44.3	−2.5	−0.4	101	2	7.2	−1.6	+0.3	234
	12	κ Capr	4.8	23	58.4	−1.8	+1.2	78	1	22.6	−2.1	+1.1	243
	18	25 Arie	6.5	8	53.8	..	..	346	9	10.4	..	..	319
	21	o Taur	4.8	5	9.6	−0.6	+1.2	83	6	12.2	−0.8	+1.8	241
	21	BD+22°925	6.5	7	3.3	0.0	+4.1	15	7	43.0	−2.9	−1.8	310
	21	175 H'Taur	6.5	12	45.8	−1.3	−0.7	84	13	52.9	−0.7	−1.2	274
	22	36 B.Gemi	6.0	3	9.4	+1.0	+2.2	26	3	38.7	−0.3	−0.2	314
	22	BD+23°1491	6.5	13	8.3	..	..	159	13	41.7	−2.6	+2.7	213
	22	87 B.Gemi	5.8	15	36.2	−0.9	−0.2	61	16	22.0	+0.4	−2.1	316
	23	187 B.Gemi	6.2	8	23.6	−1.7	−0.5	125	9	26.3	−1.5	+2.5	234
	23	82 Gemi	6.2	12	46.4	−2.4	+0.8	66	13	50.2	−0.9	−2.8	320
	24	γ Canc	4.7	10	24.0	−1.1	+1.6	17	10	33.3	−2.4	−2.7	3
	26	46 Leon	5.7	11	24.9	−1.7	+1.2	88	12	41.0	−1.6	−1.7	321

OCCULTATIONS VISIBLE IN LONGITUDE +98° 0′, LATITUDE +30° 0′*

Nov.	9	7 Sgtr	5.5	0	9.6	−3.0	−4.1	149	0	48.7	+0.1	+2.2	205
	13	κ Capr	4.8	0	50.5	−3.4	−1.1	112	1	49.0	−0.6	+2.4	195
	18	25 Arie	6.5	8	56.0	−1.0	+0.9	42	9	53.0	−0.6	−1.1	274
	21	BD+22°925	6.5	7	15.9	−2.1	+1.1	71	8	39.1	−2.3	+0.1	262
	21	175 H'Taur	6.5	13	10.3	−0.5	−0.6	80	14	6.4	+0.1	−1.1	281
	22	36 B.Gemi	6.0	2	46.6	+0.2	+1.2	71	3	41.1	−0.3	+1.0	265
	22	BD+23°1491	6.5	13	32.3	+0.2	−3.0	146	14	13.6	−1.1	+0.5	230
	23	82 Gemi	6.2	13	34.2	−2.3	+1.0	52	14	14.3	+0.5	−3.5	339
	24	γ Canc	4.7	10	48.0	−3.3	+2.8	54	11	41.1	−1.0	−4.5	342
	26	46 Leon	5.7	12	13.2	−3.0	+0.4	87	13	28.4	−1.0	−3.1	336

*Computed by Edgar W. Woolard and Paul Herget; communicated by Commodore J. F. Hellweg, Superintendent U.S. Naval Observatory.

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b;* apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

Fireball of 1934 August 30/31

On this date at 9:32 P.M., E.S.T., a fireball of at least —4 magnitude, and reddish-orange-yellow in color, was reported from two stations in New Jersey. From the map we find for S1, $\lambda = 74° 03'$, $\phi = 40° 42'$, Chas. Vogt III, observer: for S2, $\lambda = 75° 08'$, $\phi = 39° 41'$, A. P. Hess, observer. These coordinates may well be 1′ to 2′ out. S1 has a plotted path on a sketched map of Ursa Major. Transferring his positions to a globe, with such accuracy as is possible, we find $\alpha_1 = 234°$, $\delta_1 = +50°$, $\alpha_2 = 176°$, $\delta_2 = +43°$. For $\theta = 302° 32'$ we then calculate $a_1 = 125°.8$, $h_1 = 42°.5$, $a_2 = 146°.2$, $h_2 = 9°.5$. S2 states it appeared under Ursa Minor and went straight down nearly to the horizon in the N. It disappeared before it reached horizon. City lights blotted out stars in region. From this I assume: $a_1 = a_2 = 180°$, $h_1 = 35°$, $h_2 = 5°$. Plotting in the usual way, we find for S1, $H_1 = 56.9$, $H_2 = 15.9$; for S2, $H_1 = 69.7$, $H_2 = 14.4$. Weighting the observation as 3:1 we then get $H_1 = 60.1$, $H_2 = 15.5$, or 111 and 29 km, respectively. Tabular data follow:

Date	1934 Aug. 30/31 9:32 P.M., E.S.T.
Sidereal time at end point	301°.5
Began over	$\lambda = 75° 08'$, $\phi = 41° 17'$ at 111 km
Ended over	$\lambda = 75° 08'$, $\phi = 41° 55'$ at 29 km
Length of path	108 km
Projected length of path	69 km
Duration	3.5 ± 2.5 sec
Velocity	31 ± 22 km/sec
Radiant uncorrected	$a = 0°$, $h = 50°$—, $\alpha = 301°$, $\delta = -0°.5$
Zenith correction (parab.)	—3°.9
Radiant corrected	$a = 0°$, $h = 46°$, $\alpha = 301°$, $\delta = -4°$

Fireball of 1941 August 11/12

On this night at 9:21 P.M., C.S.T., a fine —4 magn. Perseid, leaving a long-enduring train, was observed by a group at the Los Estrellas Observatory, Ft. Worth, Texas, S1, and an account published in T.O.B. No. 120. Thanks to O. E. Monnig I received a fuller account shortly thereafter, along with a plot of the train. Also through him I received data submitted by B. H. Whitney at Norman, Okla., S2. Knowing further that it was a Perseid has permitted me to complete a solution.

The data at S1 are $\theta = 273° 10'$, $\lambda = 97° 16'.4$, $\phi = 32° 33'.6$, $\alpha_1 = 56\frac{1}{4}°$, $\delta_1 = +68°$, $\alpha_2 = 85\frac{1}{2}°$, $\delta_2 = +72°$. From these are calculated $a_1 = 193°.4$, $h_1 = 14°.3$, $a_2 = 183°.1$, $h_2 = 14°.6$. For S2, the data are by two students, later interviewed by Whitney, and are approximate, but serve as a check in the solution without the actual numbers being used. Observers at S2 were Bill Lyda and Burton Cossey, $\lambda = 97° 26'.6$, $\phi = 35° 12'.6$. They were looking south and turned when attracted by the bright light. $a_1 = 205°$, $h_1 = 40°$ (evidently not the beginning); ended in zenith or probably 5° to 10° N.E. of it. Brightness equal to that of full Moon, but size smaller than Moon. Brightness for first ⅓ of path. Color white with a little pink in it. Duration of train 2 to 3 minutes.

From the globe the Perseid radiant was about $a = 205°$, $h = 9°$ for S1 and $a = 205°$ and $h = 12°$ for S2. Hence if the object ended in or near the zenith, a

line drawn towards a = 205° for S2 defines the line of the projected path. Then laying off the rather accurate a_1 and a_2 for S1, the intersections give the projected path itself. Measuring from S1 to the projections of B and E gives these distances. On our scale we make 1 unit = 1′, so one-half such distances give the corrections for curvature. Adding these to the observed altitudes, through the tangents we at once derive H_1 and H_2. These are 207 and 101 km. Working out the slope of path we then get 15°.5, which is some 3° too high. However, putting in the zenith correction will make this smaller by 0°.7. I merely mention this to show that I am aware that the derived path and hence at least one height probably have a small error which there is no legitimate way of removing. Examining the plotted path at S1, it passes about 4° above the usual Perseid radiant. Lowering h_1 by 2° would cure this. It is pretty certain that H_1 is in error by a proportional amount. Fortunately the train heights depend little upon H_1, but very largely upon H_2. For the train we get a length of 81 km, its top being at 127 km and its bottom (coincident with path's end) at 101 km. The drift observed by Monnig, using binoculars, was 1° to right in 5 minutes. At the middle the train was 360± km from him. This gives a drift to east of 72 km/h. With optical aid at S1 the train was visible for some 6 minutes. This checks well with the 2 to 3 minutes at S2, but without optical aid, though only about one-third as far distant from it. While the train heights are considerably over the average for night trains yet the fact that it was certainly in or near the zenith at S2 and that h_4 was carefuly checked at S1 with binoculars, using Webb's Atlas for fainter stars to identify the train's position accurately, makes me believe that the deviation is real and not due to errors of observation. My sincere thanks are due to O. E. Monnig whose cooperation made the solution possible.

Date	1941 Aug. 11/12, 9:21 P.M., E.S.T.
Sidereal time at end point	273° 10′
Began over	$\lambda = 95°\ 36'$, $\phi = 38°\ 24'$, at 207 km
Ended over	$\lambda = 97°\ 06'$, $\phi = 35°\ 47'$ at 101 km
Length of path	347 km
Projected length of path	331 km
Velocity	347 km/sec (!!!)
Radiant uncorrected	a = 205′, h = 15°.5
Zenith correction	−0°.7
Radiant corrected	a = 205°, h = 14°.8, $\alpha = 40°.0$, $\delta = +59°.4$

Train: Length 81 km, height of top 127 km, of bottom 101 km, drift 72 km/h to E.

Fireball of 1944 April 4/5

On this date at approximately 7:45 P.M., E.S.T., a splendid fireball appeared in the west to Michigan observers, from whom only reports have come. The matter of special interest was the train which remained visible certainly until after 8:10. Thanks to two of our active observers, Bert Darling at Lansing, S1, and L. N. Upjohn at Kalamazoo, S2, excellent data on the train were received not only from them personally but, through their efforts, from several others who sent in good reports. Nevertheless, misfortune is encountered in that, though its path is by all reported as vertical, yet the difference in azimuth from S1 and S2 is only 2°.6, which is so small that very small errors of observation would vitiate the results and our usual method cannot be employed. Fortunately, from each station the altitude of the top of the cloud was carefully measured. My only possible procedure was to act on the presumption that the altitudes were accurate and, with allowance that S1 and S2 were not quite in the same vertical plane with the fire-

ball's path, a solution was attempted. Several approximations were made and the results to follow seem to fit our data quite well.

At S2 Upjohn used a telescope with circles and a compass to secure his coordinates. At S1, Darling made a series of seven drawings, using the planet Mercury as the reference point, which was easy to do as the train at first was within 1° of the planet. By calculating its position, I was able from Darling's drawings to secure a consistent set of drifts. These are in general confirmed at S2, but only two drawings were made there and these not in detail. So while confirmatory, the actual drifts depend upon Darling's drawings only. Neither of these observers saw the fireball itself. For its heights I depend upon the statement of H. I. Piatt, also at S1, who sent in an excellent report as follows: ". . . the white trail it left covered only about one-third the distance I saw the object fall. In other words, it left no trail for the top one-third nor did it leave a trail for the bottom third of its flight that was visible to me. . ." From the heights of the top and bottom of the train, a rough extrapolation will give those for the path itself. At or near S1 the fireball itself was seen by Mrs. D. C. Powell and Mrs. Edna Clark, both of whom sent in reports on it and its train. Each gives 3 seconds as the duration of flight and the former stated its brightness lay between that of quarter and full Moon. The train was also reported from S1, in detail, by H. M. Wills of the U.S. Weather Bureau and by M. E. Kester at S2. The weakness in our data is not having any reports well to north or south of S1 and S2 to check whether the path was truly vertical or merely appeared so from being almost in the vertical plane containing S1 and S2.

Date	1944 April 4/5 7:45 P.M., E.S.T.		
Began over	$\lambda = 88°\ 08'$ W, $\phi = 43°\ 22'$ N at 71 km		
Ended over	$\lambda = 88°\ 08'$ W, $\phi = 43°\ 22'$ N at <20 km		
Sidereal time at end point	113°.3		
Radiant (no correction)	$\alpha = 111°$; $\delta = +43°$		
Train; top of	47 km		
Train; bottom of	23 km		
Train drift to south at	A	47 km	at 0 km/h
Train drift to south at	B	36	45
Train drift to south at	C	34	0
Train drift to south at	D	33	51
Train drift to south at	E	25	30
Train drift to south at	F	23	30

The train drifts refer to the interval 7:55 to 8:05. They are of course *projected* velocities. The train was at first straight, then twisted into a spiral. At 7:55 the point E was already definitely north of the average, but from then on it too drifted south. I consider that there were six definite horizontal currents. What are shown are the maximum and minimum southerly drifts. Anywhere between successive points the velocity would be roughly proportional to the fraction of the vertical distance from point to point. The train was very bright: Darling reports the part B, the most conspicuous, to be of "zero magnitude" at 8:07; at 8:10 only about 1½° was left, but it was still "first magnitude." Its maximum was at 8:01 when it attained —2 magnitude, due to "doubling back upon itself." The sky was perfectly clear, practically no ground wind—and very cold.

As for the accuracy of our computed results, it seems that the velocities of drift and certainly their projected directions are quite good. The radiant is based upon the path being vertical. Any slope, not seen due to projection, would mostly affect its right ascension. The heights of the train are within expected limits of

error for the average of day trains: this came in late twilight. For any such low end point meteorites should have fallen. They would be in Washington Co., Wisconsin, if our results are exact.

FIREBALL OF 1944 APRIL 9/10

Approximately at 7:11 P.M., E.S.T., an unusual fireball crossed part of New England and ended over New York. Unfortunately, of the five stations from which reports came, it was evident that clouds, etc., prevented the whole path from being seen at any one. They and the observers were: S1, Lawrence, Mass., H. S. Noyes; S2, Westminster, Mass., W. R. Heins; S3, Danbury, Conn., H. W. White; S4, Amytiville, L. I., N. Y., Dr. F. S. H. Adamo and others; and S5, The Bronx, K. Weitzenhoffer. It was impossible to reconcile all of the data, due, I believe, to some misunderstanding as to how coordinates should be expressed. The solution of the heights depends on S3, S4, and S5; S1 and S2 are used in determining the azimuth of the path only. The fireball exploded into at least two fragments and was very brilliant. Some evidence points to its having been fainter at the very end. It was bluish-white in color. I regret to say that little confidence can be placed in the derived path. It is certain that it was about horizontal and from east to west. The azimuth of the radiant was 252° ± and the path 313 km long at a height of 50 ± 8 km, lying probably 20′ north of S3. For the last half of the path we find an observed velocity of 22 km/sec, based upon estimates at S4 and S5. This is a low velocity, but atmospheric resistance for a horizontal path would adequately explain it. In addition to his data, Dr. Adamo sent a detailed drawing of his conception of the path in space and also a colored sketch, which latter I am sorry not to be able to reproduce. Our thanks go to all the five observers, but most of all to him as he took the trouble to telegraph his first report and took much time in preparing his final and more nearly complete one.

Contributions of the Society for Research on Meteorites

Edited by **FREDERICK C. LEONARD**
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An Electromagnetic Cane for Meteoriticists

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ABSTRACT

The use of highly sensitive meteorite detectors may be impossible under field conditions readily permitting the recovery of surface meteorites by the use of a magnetic cane. Even when conditions are favorable for the use of powerful and elaborate detecting equipment, it may be desirable to employ a magnetic prod to test the nature of objects located beneath the surface of the ground. It is the purpose of this paper to describe an electromagnetic cane or prod which has proved most useful, not only in visual searches for surface meteorites but also in the identification of subsurface objects located by non-discriminatory meteorite detectors.

There are certain field conditions under which it is impossible to use powerful and highly sensitive meteorite detectors. Those who have tried to work in windy weather at the Odessa, Texas, and Canyon Diablo, Arizona, craters or at Arntz (near Holbrook), Arizona, will fully appreciate this statement. When use of the more elaborate meteorite detectors is impossible, the ardent meteorite hunter is sure to attempt a visual search, even in regions such as the environs of the Canyon Diablo Crater, which have already been systematically worked over by many investigators. Such visual searches afford most welcome relief from the physical and nervous strain of carrying and properly operating even the lightest and most simply designed meteorite detectors. It is really surprising how exhilarating is the friendly rivalry developed in a group making a visual search, as those who participated in the meteorite hunt which was a feature of the last, the Eighth, Meeting of the Society for Research on Meteorites, at Flagstaff and Canyon Diablo, in June, 1941, can testify. Nevertheless, no one can deny that there are few more back-breaking tasks than continually bending down to examine objects suspected of being tiny surface meteorites!

Not only in visual relaxation sweeps but in many other connections need arises for a magnetic cane or prod which will save the meteorite hunter from arduous physical exercise and will increase his ability to distinguish the truly meteoritic from the false material, regardless of whether the objects tested lie on the surface or are buried at a shallow depth in sand or other surface drift. The artifice of mounting a small horseshoe magnet of alnico on the end of a stick presents a partial solution of this problem, for such permanent magnetic devices have been used with success, not only to pick up tiny surface fragments of the Canyon Diablo iron but also to drag forth from arenaceous concealment the tiniest of the Holbrook stones so far recovered.[1] However, the use of alnico horseshoes affords only a partial solution, for, on the one hand, the very shape of the magnet and the flimsy manner in which it is habitually fastened to the end of the stick both preclude use of the device as a magnetic prod to be pushed straight down several inches into the meteorite-bearing topsoil; and, on the other hand, the alnico magnets, strong as they are, cannot match the powerful magnetic fields developed about suitably designed and energized electromagnets.

How important the power of the magnetic field is to the meteoriticist may be inferred from the fact that all of the first 4 Holbrook "micro-meteorites" were recovered by the use of the *stronger* of 2 alnico magnets (3-in., 1¼-in.) used under otherwise essentially identical conditions. Again, a strong, slender prod, which can be magnetized and used to test the nature of subsurface objects located by the use of non-discriminatory meteorite detectors, such as the standard "perpendicular coil" instruments,[2] would be obviously a most useful adjunct in the field. Employment of such a magnetic prod in the sandhills where the Monahans, Texas, meteorite was found would enable the meteoriticist speedily to distinguish between buried irons and old Indian water basins scraped out of hard rock and holding moisture, even tho drifted over by a foot or more of dry sand. Of course, such a magnetic test-prod is not required by one using a meteorite detector which discriminates between substances giving rise to distortion of the magnetic field of the detector (*i.e.*, ferromagnetic substances, *e.g.*, iron meteorites) and those producing a signal in the detector by means of eddy-current effects (*i.e.*, non-ferromagnetic substances, such as moist soil). However, it appears that the only meteorite detector possessing this discriminatory power is the "imaginary top" detector recently described by F. O. Wisman and so far but little

used in the field.[3] Finally, it may be noted that, in view of the strategic importance of alnico and the scarcity of certain of the elements of which this alloy is compounded, considerable difficulty may be anticipated in securing powerful alnico or other permanent magnets for use in meteorite hunts for some time to come.

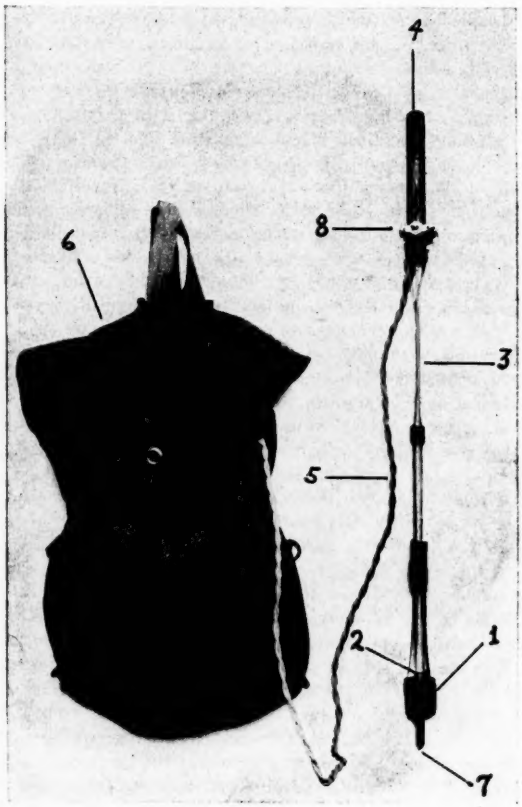

FIGURE 1

AN ELECTROMAGNETIC CANE FOR METEORITICISTS

It is the purpose of this paper to describe an electromagnetic cane or prod which has proved most useful in a variety of field situations. This cane, shown in the accompanying photograph (Fig. 1), consists of a small coil (*1*) of No. 20 enameled wire wound on a brass tube (*2*) of diameter 3.5 cm. and length 4.0 cm., which can be slid up and down on a light, but strong, iron rod (*3*) of diameter 1 cm. and length 50 cm., provided with a comfortably shaped wooden handle (*4*). In the photograph, the coil is shown in the position proper for pick-

ing up surface meteorites; that is, the base of the coil is about 4 cm. above the end of the iron rod. The portion of the rod which projects below the coil appears black in the photograph, since the light-colored coating which shows above the coil has been worn off by the abrasive action of sand grains. For test probing in deep sand, it may be necessary to set the coil much higher up on the iron rod than shown in the photograph. The electromagnet is connected by leads (*5*) to a power source carried in the knapsack (*6*). If a sufficiently small battery is employed as a source of current, the electromagnetic cane can be used continuously without overheating, but of course the resulting magnetic field is weak. If stronger fields are desired, it is necessary to use only more powerful batteries; *e.g.*, when using a Burgess uniplex, light-weight, heavy-duty, 6-volt battery (No. 4F4H), the electromagnetic cane shown in the photograph will readily pick up an Odessa iron weighing more than one pound and will cause smaller meteoritic fragments to jump an inch or more to the collecting tip (*7*). The heating of the coil when driven by this uniplex battery is not excessive but is sufficient to require occasional rest periods. However, to secure very powerful electromagnetic fields, the coil must be energized by batteries so strong as soon to overheat the coil under continuous operation. In order to avoid such overheating, a toggle switch (*8*) is mounted on the wooden handle, and the circuit thru the coil is kept closed only for the very brief periods necessary to pick up surface meteorites or to identify subsurface objects.

REFERENCES

[1] Nininger, H. H., *C.S.R.M.*, **2**, 258-60; *P. A.*, **49**, 159-62, 1941; and Leonard, F. C., *C.S.R.M.*, **2**, 288-91; *P. A.*, **49**, 384-7, 1941.

[2] La Paz, L., *C.S.R.M.*, **3**, 9-17; *P. A.*, **50**, 157-65, 1942.

[3] Wisman, F. O., *C.S.R.M.*, **3**, 18-25; *P. A.*, **50**, 215-22, 1942.

1945 August

Some Meteoritic Iron Sulfides

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ABSTRACT

Two varieties of iron sulfide from one of the Gibeon (Bethany), Great Namaqualand, South Africa, irons are described and analyses are given. One variety is dark in color, soft, nonmagnetic, and nickel-free, while the other is lighter-colored, hard, polishable, strongly magnetic, and exceedingly rich in nickel. An analysis of a mixture of iron sulfide and daubréelite from another of the same group of meteorites also is given. Sulfides from Canyon Diablo, Arizona, and Xiquipilco (Toluca), Mexico State, Mexico, also are described and analyses for these too are given.

Evidence has been accumulating for some time that there is more than one kind of meteoritic iron sulfide. Thus, Nininger reported that the sulfide of the Chihuahua City, Chihuahua, Mexico, iron existed in 2 forms.[1] One was brassy, polishable, and acid-resistant; the other was blackish, powdery, and readily attacked by acid. Neither was analyzed. Some years ago, I showed that there is evidence for the existence of a sulfide having the formula FeS, *i.e.*, troilite, and another with a formula near $Fe_{10}S_{11}$, *i.e.*, pyrrhotite.[2] Stillwell,[3] Baker and Edwards,[4] and Hodge-Smith and Edwards[5] have recently described certain Australian meteorites and distinguished 2 varieties of sulfide in them. The stones

contained "pyrrhotine" and the irons contained both "pyrrhotine" and troilite. The troilite is attacked by dilute acid, but "pyrrhotine" is only slightly attacked, if at all. In my own experience, I find that different samples of sulfide differ in their acid-resistance, but, when analyzed samples are used, there is no clear relationship between acid-resistance and chemical composition; thus, of 2 sulfides with the composition of troilite, one resisted dilute acid and one did not; similarly, 2 terrestrial pyrrhotites showed a similar difference in resistance.

I have in my collection a slice of the Gibeon or Bethany (Mukerop), Great Namaqualand, South Africa, iron. One 17-mm. nodule is composed obviously of 2 substances. One substance is bronze-brown, hard enough to take an excellent polish, strongly magnetic, and very rich in nickel. It is readily attacked by dilute nitric acid and occurs alone in small scattered nodules as well as in irregular masses in the large nodule. It gives a dark gray powder. The other sulfide is nearly black, too soft to polish, nonmagnetic, resistant to dilute acid, and completely free of nickel. It gives a black powder and forms the greater part of the large nodule. Both kinds of sulfide were subjected to analysis with the following results:

TABLE 1

	Magnetic	Nonmagnetic
Fe	52.3%	61.2%
Ni	8.4	nil
S	39.3	38.7
P	—	trace
	100.0%	99.9%

Both analyses correspond to that of pyrrhotite.

These sulfides can hardly be simple varieties, because, in addition to their other differences, they are insoluble in each other. The magnetic form must be completely insoluble in the other, because the nonmagnetic sulfide contains no trace of nickel. The latter, likewise, is probably insoluble in the magnetic form, because, if it were soluble, the nickel content of the magnetic sulfide would be reduced; actually, it contains more nickel than has ever been previously reported. Furthermore, any appreciable mutual solubility is precluded by the occurrence of these sulfides as 2 separate entities in contact. Their arrangement in the nodule is about what one would expect from the solidification of 2 immiscible liquids of nearly the same specific gravity. Since they are mutually insoluble, they cannot have been isomorphous at the time of their solidification, and therefore they have probably different crystal structures. This conclusion is supported somewhat by the fact that the 2 sulfides form a thermoelectric couple. At 26° C., the magnetic form is positive with respect to the other. The only chemical differences are the presence of nickel in the magnetic form and a slight difference in the excess of sulfur. The formulae of the magnetic and nonmagnetic forms are respectively Fe_4S_5 and $Fe_{10}S_{11}$. The latter is obviously close to troilite.

The high nickel content cannot be ascribed to admixed metal or schreibersite, because the necessary quantities would be much too large to escape detection. The largest, previously reported, nickel content was in Taylor's analysis of the Xiquipilco (Toluca), Mexico State, Mexico, troilite.[6] He found 6.70% but did not comment on its magnetism. Sjöström found 4.34% in the sulfide of the Beaconsfield mass of the Cranbourne, Mornington County, Victoria, Australia, fall.[7] Other samples of the sulfides of both of these falls contained less than 1% of nickel. The magnetism of one of the sulfides described here and that of the Bea-

consfield sulfide may have been due to the nickel content. This conclusion is supported by the fact that terrestrial nickeliferous pyrrhotite is usually strongly magnetic, while most meteoritic sulfides, terrestrial troilite, and even some pyrrhotites are nonmagnetic.

The magnetic sulfide does not contain enough nickel to be pentlandite, and, in any case, pentlandite is soluble in pyrrhotite above 450° C. This fact suggests that the magnetic sulfide may be a new meteoritic mineral, since it does not appear to be pyrrhotite or any other previously known iron-nickel sulfide. There is a possible alternative explanation. I have a slice of the Xiquipilco (Toluca) iron which contains an elongated troilite nodule. At one end of this is a sharply defined area rich in some black mineral mixed with the sulfide. On the other side of the slice, the nodule has a different shape and occupies a different position, as tho a nodule of this composition had been formed and later a purer sulfide had been deposited over parts of the surface of the nodule in a very irregular manner; thus, this nodule bears a certain resemblance to the Gibeon nodule. In the Gibeon nodule, however, there is no telling which, if either, of the components was deposited first, and tho their interface is sharp, they are intergrown in a very complex manner. The possibility remains, however, that 2 sulfides may have been deposited at different times and that one contained nickel while the other did not. Altho pentlandite is soluble in pyrrhotite above 450° C., "unmixing" may occur on cooling, provided cooling begins from some temperature above 800° C.[11] The nickeliferous sulfide could therefore be a mixture of pyrrhotite and pentlandite. Microscopic examination of the Gibeon nodule indicates that the magnetic sulfide is quite homogeneous, but that small nodules which appear macroscopically to be the same material do show veinlets of a lighter-yellow mineral. If these veinlets are pentlandite, they appear to be considerably less in quantity than the 25% calculated from the analysis. If this explanation is the true one, then this is the first instance of the occurrence of pentlandite in a meteorite. It should be noted, however, that this explanation does not explain the magnetism of the nickeliferous sulfide, because pentlandite is not magnetic. It fails to explain also the greater hardness and polishability, because pentlandite is, if anything, slightly softer than pyrrhotite.

I have to report 2 other analyses of meteoritic iron sulfides as follows:

TABLE 2

	Canyon Diablo	Xiquipilco
Fe	65.85%	60.75%
Ni	nil	3.42
Co	nil	n.d.
S	34.15	35.85
P	trace	trace
	100.00%	100.02%
Insoluble	16.33%	3.74%

Both of these analyses refer to troilite with a slight excess of iron. Both materials were nonmagnetic and resistant to dilute nitric acid. The specific gravities were 4.445 and 4.910, respectively. (Another unanalyzed sample from Canyon Diablo gave 4.385.) Both were very dark in color, closely resembling the nonmagnetic sulfide described previously. The insolubles were both black powders, presumably graphite.

Altho sulfide is abundant and occurs sometimes as nodules of large size in Canyon Diablo irons, the foregoing analysis seems to be the first complete analysis

of the sulfide of this fall. This fact is difficult to understand, inasmuch as these irons are, or were, very plentiful and otherwise unusually well described. The only other analysis is a sulfur determination by H. G. Byers.[8] He found 37.32%, but of particular interest is his discovery of 0.0023% and 0.0200% of selenium in this sulfide. The Noddacks reported 0.084% of selenium in a composite sample of troilites.[9]

A sample of sulfide from an Odessa, Texas, iron had a specific gravity of 3.58, which is low, probably because of the 52.4% of graphite which it contained. It was nickel-free and nonmagnetic. It gave a strong reaction for phosphate and contained magnetic particles. Since no nickel could be detected, this fact may indicate the presence of a nickel-free phosphide or possibly some unknown phosphate. There is other evidence for the existence of some phosphate mineral in troilite; *e.g.*, I have previously reported a positive phosphate reaction in the water extract of the troilites of Xiquipilco and Canyon Diablo.[10] Free sulfur also was found in very small amounts in these sulfides. The Odessa troilite contained only a doubtful trace of free sulfur. Much combined sulfur and iron were found, but, on account of an accident, the results were not sufficiently accurate to report.

Recently I attempted to ascertain the ash content of a sample of alleged graphite from another of the Gibeon (or Bethany) irons (Amalia Farm). After heating to a high temperature for several hours, the weight loss was only 11.56%, and the material was then at constant weight. Accordingly a sample was submitted to Mr. F. G. Hawley for analysis. He obtained the following results:

TABLE 3

Fe	56.90%
Ni	0.09
Cr	3.38
S	30.70
SO_3	2.21
P	trace
Insol.	0.20
Graphitic Carbon	nil
Organic oil, O_2, H_2O	present
	93.48%

This analysis represents: troilite, 73.00%; daubréelite, 18.72%; iron sulfate, 4.26%; and iron oxides, 4.02%. No doubt the iron sulfate and part of the oxide resulted from oxidation of the troilite during the heating. Another part of the oxide came probably from metallic iron, since this sulfide contained magnetic particles, and, in a humid atmosphere, cracked open, thus suggesting the presence of metallic iron and lawrencite. The material is dark reddish-black and, for the most part, dull and lusterless. Probably it is an intimate mixture of troilite and daubréelite, and the black color is due largely to the latter. This is the material described as "Amalia Farm graphite" in a previous paper,[10] in which the presence of water-soluble chloride and sulfate was reported, together with 0.000x% of free sulfur. The chromium in the preceding analysis does not represent chromite, since the mineral is nearly all soluble in nitric acid.

REFERENCES

[1] H. H. Nininger, *Am. Jour. Sci.*, **22**, 69, 1931.
[2] J. D. Buddhue, *C.S.R.M.*, **1**, No. 3, 38-40; *P. A.*, **45**, 386-8, 1937.
[3] F. L. Stillwell, *Mem. Nat. Mus., Melbourne*, No. 12, 41-7, 1941.
[4] G. Baker and A. B. Edwards, *ibid.*, pp. 49-58.
[5] T. Hodge-Smith and A. B. Edwards, *Rec. Australian Mus.*, **12**, 1-8, 1941.
[6] W. J. Taylor, *Am. Jour. Sci.*, **22**, 374-6, 1856.

[7] O. Sjöström, *Sitzb. Berlin. Akad.*, p. 1044, 1897.
[8] H. G. Byers, *Ind. and Eng. Chem., News Ed.*, **16**, 459, 1938.
[9] W. and I. Noddack, *Die Naturwiss.*, **18**, 759, 1930.
[10] J. D. Buddhue, *Am. Min.*, **26**, 679, 1941.
[11] R. L. Hewitt, *Econ. Geol.*, **33**, 317-18, 1938.

Notice concerning the President of the Society

Dr. Lincoln La Paz, Professor of Mathematics at the Ohio State University and President of the Society for Research on Meteorites, has accepted appointment, as of date November 1, 1945, as Professor and Head of the Department of Mathematics and Director of the recently created Institute of Meteoritics of the University of New Mexico at Albuquerque. Dr. La Paz was a member of the faculty of the Ohio State University from October, 1930, to October, 1945. Since January, 1943, he has been on leave of absence, serving as Research Mathematician for the Office of Scientific Research and Development. Since January, 1944, he has been also Technical Director of the Operations Analysis Section of the Second Air Force.

President of the Society: Lincoln La Paz, Department of Mathematics and Institute of Meteoritics, University of New Mexico, Albuquerque
Secretary of the Society: C. H. Cleminshaw, Griffith Observatory, P. O. Box 9866, Los Feliz Station, Los Angeles 27, California

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Nova Aquilae (Tamm) 1945: Word has been received of the discovery by Dr. Nils Tamm on August 28 of another nova in the constellation Aquila. The visual magnitude at the time of discovery was reported as 7.0, and the photographic as 8.5. Visual estimates made at Harvard on the evening of August 29 gave an approximate magnitude of 7.2, while the photo-visual magnitude that same night was 7.1.

Subsequent visual observations by A.A.V.S.O. members, who were promptly notified of the discovery, indicate a steady decline in light to magnitude 8.2 on September 6, while the photographic magnitudes determined at Harvard show that the nova had decreased from magnitude 7.2 to 8.5 between August 27 and September 3. An examination of Harvard pre-discovery plates reveals that the nova was at least fainter than the 16th magnitude on exposures made between the years 1901 and 1917, and that the outburst must have occurred between August 17 and 27, 1945. On the first-mentioned date, the nova was not visible, fainter than magnitude 10.8, and on the last date, one day before discovery by Tamm, it was of magnitude 7.3. On the whole, the nova is about a half to three-quarters of a magnitude fainter photographically than visually. D. B. McLaughlin concluded, from an examination of the spectrum taken on August 29, that the nova was then in the post-maximum stage, and about 1.0 to 1.5 magnitudes below maximum brightness. J. A. Pierce, also from a study of the spectrum, estimated the distance of the nova as 2,550 parsecs, and of absolute magnitude, after correction for space absorption, as —6.8.

Aquila appears to be a favorite *habitat* for novae, with at least seven known to date, and five of these located in an area one degree in right ascension and two degrees in declination. Tamm has discovered three of them, two in 1936 and the present one. Such a region might well be the special field for research by the A.A.V.S.O. Nova Searchers, especially with the aid of field glasses or small telescopes.

Recent Observations of SS Cygni: In the August, 1943, issue of these *Notes* the observations of SS Cygni for the year 1942 are tabulated and plotted. The plotted observations from 1942 to the present time (JD 2430700 to 2431700) are

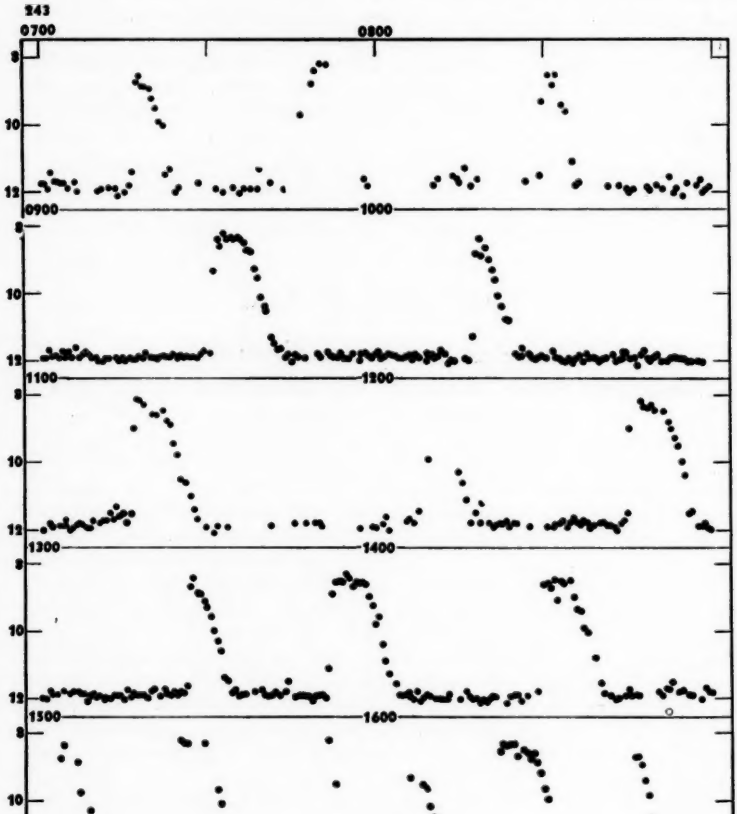

FIGURE 1

LIGHT CURVE OF SS CYGNI,
1943-1945 (J.D. 2430700-1700).

shown in the present note. The particular feature to be observed is the great diversity of intervals between successive maxima, from less than one month to more than three months, with an average of 60 days. This feature is indicated not only in the plotted light curves, but also in the appended table of observed maxima, under the headings "Diff."

In the table are listed the seventeen maxima observed during these 1,000 days, with the classifications as to type, the Julian dates when the star attained the tenth magnitude on the rise to maximum, the approximate dates of maximum, and the dates when the star reached the tenth magnitude on the declining slope of the curve. The current number assigned to each maximum is reckoned from the first maximum observed in 1896, after the star was found to be variable, at Harvard. As far as can be determined only *one* maximum has escaped detection since the star has been under close visual observation, probably in February, 1941, when the star was unfavorably placed for observation.

The average length of a cycle over this interval of nearly fifty years is 51.4 days with widely separated extremes of 20 days and 110 days, as indicated above. It will be noted also that the majority of these seventeen maxima are of the A-type (rapid rise), with only two of the B-type (less rapid rise), and only one of the C-type (moderately rapid rise).

SS Cygni continues to be the favorite variable star with amateur observers, and is undoubtedly the most completely observed of all variables.

OBSERVED MAXIMA, 1943-45

No. Max.	Type	J.D. 10.0 I	Diff.	J.D. Max.	Diff.	J.D. 10.0 D	Diff.
330	A3	2430728	97	2430730	92	2430736	89
331	C4	0777	49	0784	54	0792±	56
332	A2	0849	72	0853	69	0857	65
333	A8	0951	102	0956	103	0966	109
334	A3	1029	78	1030	74	1036	70
335	A7	1127	98	1129	99	1141	105
336	B2	1215	88	1219±	90	1223	82
337	B8	1277	62	1280	61	1290	67
338	A4	1344	67	1345	65	1351	61
339	A7	1386	42	1391	46	1401	50
340	A7	1448	62	1454	63	1462	61
341	A3	1505	57	1508	54	1513	51
342	A6	1540	35	1545	37	1553	40
343	A1	1584±	44	1585	40	1589	36
344	A1	1609	25	1612	27	1616	27
345	A8	1635	26	1642	30	1641	25
346	A1	1675	40	1676	34	1681	40

The Variable Star VZ Camelopardalis: Mr. Joseph Ashbrook has been observing the variable star VZ Camelopardalis and submits the following report. The star is a semiregular variable with a giant M4 spectrum. The variability was established photoelectrically by Stebbins and Huffer two decades ago, but no detailed study of the form of the light variations has ever been published. A series of 450 visual observations was begun in 1943 and is being continued; it shows that the star has a persistent period of 24 days, which is believed to be the shortest yet established for an M-type semiregular variable. The limits of light variation are from 4^m7 to 5^m2 (in the HPv system) but the average range is between 0^m3 and 0^m4. The star bears a considerable resemblance in light variation, spectrum, and absolute magnitude to R Lyrae, Z Leonis, and ρ Persei.

Mr. Ashbrook suggests that the star might be a good candidate for observa-

tion by the A.A.V.S.O.; the two comparison stars which he is using are:

Star	Position 1900	Mpv	Spectrum
b	$6^h\,45^m5 + 77°\,6'$	4.62	K5
c	$6\ 24.9 + 78\ 4$	5.61	K0

Because of the small amplitude it is further suggested that estimates be made by the Argelander step method, to insure maximum accuracy, and that observations should be made on each available night over an interval of several months. An opera or field glass should preferably be used.

Nova (T) Pyxidis: In *Variable Star Notes* for June, attention was called to the fourth outburst of Nova (T) Pyxidis, noted by Dr. Joy at Mount Wilson Observatory, who found the nova to be about three magnitudes above normal minimum brightness, or about the eleventh magnitude.

At that time it was concluded that the nova was well on its downward course to minimum, and it was hoped that some earlier plates might have been secured in the southern hemisphere which would indicate just when the maximum occurred and what brilliance the star attained.

A recent communication from Dr. J. S. Paraskevopoulus at Bloemfontein gives photographic observational data from November 21, 1944, to March 9, 1945; during this interval the nova decreased in brightness from magnitude 7.1 to 9.9.

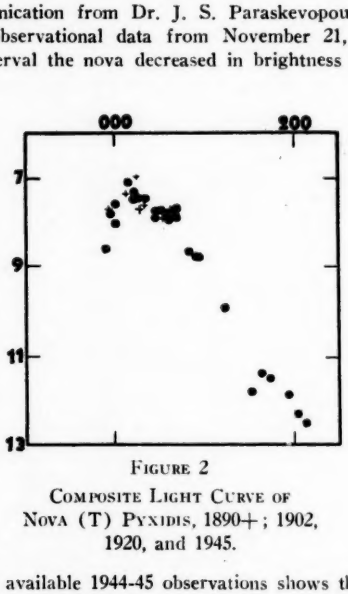

FIGURE 2

COMPOSITE LIGHT CURVE OF NOVA (T) PYXIDIS, 1890+; 1902, 1920, and 1945.

A plot of all the available 1944-45 observations shows that the nova reached the seventh photographic magnitude at maximum, approximately the same as was attained in 1902 and 1920; apparently the nova was at maximum brightness in its most recent outburst at about the time when the first Harvard plate was taken.

A composite light curve is shown in Figure 2, with maxima indicated for 1890, April 8; 1902, April 30; 1920, April 9; and 1944, November 21.

Observations for July and August: A total of 5140 observations—2106 in July and 3034 in August—were contributed by 46 observers, as listed herewith.

Observer	July—1945—August No. Var.	No. Ests.	No. Var.	No. Ests.
Bappu, M. K. V.	21	123	..	..
Bell	6	7	9	19
Blunck	55	62	52	63
Boone	12	12	19	24
Bouton	7	8	12	16
Buckstaff	11	24	17	61
Chandra	109	134	..	..
Cilley	25	35	..	..
Cousins	29	50	43	50
Cragg	31	38	32	50
Dafter	..	..	10	49
Dartayet	1	7	..	..
Duffie	38	43	27	31
Fernald	236	593	253	823
Ford	..	..	5	8
Gemberling	13	13	..	..
Halbach	27	33	..	..
Harris	11	11	32	32
Hartmann	81	86	94	99
Howarth	..	..	14	14
Hukill	2	20	2	18
Kearons	..	..	74	152
Kelly	13	13	11	13
de Kock	74	195	73	271

Observer	July—1945—August No. Var.	No. Ests.	No. Var.	No. Ests.
Koons	20	29	26	37
Luft	18	164	23	261
Mary	14	28	20	21
Mount	..	..	8	11
Nadeau	71	107	103	156
Oheim	37	51	102	145
Oravec	6	26	7	30
Parker	20	20	..	..
Parks	22	29	14	24
Peltier	96	139	155	192
Reeves	..	..	2	3
Renner	..	..	65	70
Rosebrugh	9	43	9	56
Schoenke	4	5	29	29
Segers	..	..	17	49
Sill	..	..	94	100
Stowe	..	..	11	18
Taboada	..	..	1	1
Treadwell	10	15	..	..
Webb	16	17	..	..
Weber	43	43	38	38
Weitzenhoffer	2	6	..	..
46	Totals	2106		3034

September 11, 1945.

Comet Notes

By G. VAN BIESBROECK

Last month several comets were under observation but only one was in reach of moderately sized instruments, PERIODIC COMET KOPFF, which has been followed since early last May. Its brightness increased as predicted during the summer and reached a maximum of 8.6 on the Harvard scale in the first days of August. It appeared then as a round coma nearly 4′ in diameter with a sharp central nucleus. This was near the time of perihelion which occurred on August 11. The distance is now increasing and the brightness dropping. The latest record here was 9^m1 on September 1, as estimated extrafocally by comparison with neighboring stars. This object will remain visible for another couple of months but it will soon become fainter. Kepinski's ephemeris is continued here.

EPHEMERIS OF PERIODIC COMET KOPFF

1945	α h m	δ ° ′
Oct. 2	18 34.5	—14 46
12	19 3.4	14 16
22	31.7	13 33
Nov. 1	19 59.2	12 36
11	20 25.8	11 27
21	20 51.4	10 8
Dec. 1	21 16.0	8 40
11	21 39.7	7 5
21	22 2.4	5 24
31	22 24.3	— 3 39

After several futile trials COMET 1944 *b* (VÄISÄLÄ) was finally located by the writer on August 7. It stood nearly a degree away from the computed posi-

tion and was considerably fainter. It appeared as a diffuse spot with little condensation and I estimated the magnitude as 15.5 The recovery will make it possible to follow the comet for some time longer although it gradually becomes fainter.

It will be remembered that this comet was found by the Finnish astronomer on 1944 April 18 and that there was much delay in forwarding the notice of its discovery. However, two measures, 1944 August 7 (Yerkes) and August 12 (Lick), shortly before the object was lost in the evening sky enabled P. Herget to establish an approximate orbit giving 1945 January 3 for the time of perihelion. The comet remained for several months in the vicinity of the sun after which the far southern declination made observations difficult. Since the measures now cover almost a year and a half it will be possible to determine a reliable orbit.

Periodic Comet Pons-Winnecke was last seen here on August 4. At very low altitude in the evening sky the magnitude was evaluated as 13, a figure which is rather uncertain. Unless further observations have been secured in the southern hemisphere they will have covered a three-months' interval at this unfavorable apparition.

The extremely faint Periodic Comet Oterma could hardly be recognized on plates taken by the writer in August, which means that it was hardly as bright as 17th magnitude.

Y. Bobone at Cordoba (Argentina) gives an improved set of elements for the second Comet du Toit of this year. It has been well followed in the southern hemisphere but is too unfavorably situated for northern observers.

Periodic Comet Schwassmann-Wachmann 1925 II now reappears as a morning object. Observations of its changing brightness are of interest. It will be found close to the position indicated by the following ephemeris by P. Herget:

1945	α h m	δ ° ′	1945	α h m	δ ° ′
Sept. 30	5 16.5	+32 32	Nov. 17	5 5.7	+33 4
Oct. 16	15.7	32 49	Dec. 3	4 57.8	32 57
Nov. 1	12.0	+33 01	Dec. 19	4 49.6	+32 39

Williams Bay, Wisconsin, 1945 September 10.

Communications and Comments

Under this heading we shall publish from time to time such material as does not properly fall under any of the established headings of this journal. Here, too, may be found, when occasion arises, articles which the editor may not be willing to give sanction to but which, nevertheless, may be provocative of thought along new lines.

Stalag Luft No. 1—Germany

Yes, that was John's address, Prisoner of War 0-733226, Stalag Luft No. 1, Germany. The first message read, "Regret to inform you your son John . . . missing over Germany . . ." People said his mother should not grieve so; John is just another boy, they added, the same as thousands of others. No, John's mother couldn't think that way. "My John," she cried, "Why did it have to be my Johnny?"

Long ago God told one of his poets to say, as if to assuage these tearful

souls:

I feel that, in the Heavens above
The angels, whispering to one another
Can find, among their burning terms of love,
None so devotional as that of "Mother."

This story about John started thirteen years ago. Johnny lived near Allegheny Observatory, which is situated on the top of the highest hill in Riverview Park, Pittsburgh, Pennsylvania. The activities in the park itself are carried on through a body called the Riverview Park Commission. John would go to the park and to the observatory with the writer of this article, along with other neighborhood children. It was in the 1932-1933 season that Leo J. Scanlon encouraged me to start summer outdoor classes in amateur astronomy, the study of constellations, and the orientation of navigational stars. We held our gatherings one night a week on a large plateau just above the Riverview Park Nature Cabin. During inclement weather, lectures with slides were given in the Cabin auditorium. The first year over two hundred registered. We held these young people with a broad program, and the meetings continued to be popular as time went on. Later, of course, the war took away many of the boys, including more than one John. These Johns, Carls, Charlies, and others became fliers, navigators, bombardiers, commanders, radio men, and filled all ranks of the service.

When the United States was preparing for war, we were called upon especially to interest young fellows as prospective Aviation Cadets, to teach them the fundamentals of celestial navigation and the mechanics of aviation. About this time, the classes were transferred from the Nature Cabin grounds to Allegheny Observatory, where the program could be livened up through the use of the 13-inch refractor and lectures given by Z. Daniel, astronomer in charge of the Monday night meetings. The attendance rose and fell as the demands for Air Cadets changed. Many of the boys also joined the classes being started at the Buhl Planetarium where they had the advantage of laboratory study and practice, including the celestial co-ordinate system, star identification, use of the sextant, use of the air and nautical almanacs, and reduction of observations.

But let's get back to John. He was one who liked to see the large Copernican "planetelle" work at Allegheny Observatory and to watch the stars pass overhead at the Planetarium Show. The other John (Big John) from across our street often came over to borrow my "Bowditch" because he was not permitted to take his "Seaman's Bible" away from the classroom at Pitt! And Charles from next door would say, "Let me point the telescope; I can see it." Charles did have excellent young eyes, always the first to see the stars at dusk.

John became a flier and could hardly wait until his crew was shipped out. He was an instructor at several camps and did not get to go right away. It was hard for John to see some of his schoolmates leave while he had to stay on that instruction work. Charles didn't fare so well, either. He was just too skilled in passing on his knowledge of instrument details to other men in the school. Big John from across the street had better luck and was soon on the high seas. But eventually Johnny went to England, Charles went to Hawaii, and Big John to the South Pacific; all proud to go, anxious "to get into it."

So, we carried on as best we could. New boys came along and then they, too, joined the others. Each year it was the same, and we have started our Monday night gatherings again this spring at Allegheny Observatory. Dr. Wagman, Dr. Burns, and Mr. Z. Daniel, are most kind and considerate in keeping the program alive and interesting. The young chaps are still air-minded just as much

as the others before them, and want to learn to identify navigational stars. The lectures given by Mr. Daniel are varied and educational. The observatory has one of the finest lantern slide collections, pertaining to astronomy, in the world. Recently, some new slides have been added, including a lot showing the old "Pawnee Sky Map" made over three hundred years ago by the Pawnee Indians, depicting the location of many first, second, third, and fourth magnitude stars, constellations of all seasons, and the Milky Way. Yes, these programs of thirteen years duration have borne fruit; they have brought many pleasant evenings to young and old. Even romance was not to be denied, for Leo Scanlon will now admit that when we were all chasing Sagittarius across the southern sky, he and Margaret (now his wife) were slyly holding hands and pretending to be interested in thoughts of other things than love! The Amateur Astronomers Association of Pittsburgh, with its enthusiastic members, have continuously helped our programs along in every way possible.

But somebody is probably wondering, what about Johnny and Stalag Luft No. 1. On a cold December night, a heavy squadron of bombers started from Northern France on a mission into Germany. It was blasting away for the big "push." John was navigator in the lead plane; the Colonel took John along because he "knew his navigation." The reception they got was terrific; the flak and anti-aircraft fire extremely heavy. Not all of the planes came back. The lead ship was missing. Several crews reported back at base that they saw the Colonel's plane drop her wheels and go down. Nobody could say she landed safely. John's mother heard these things later, after the first message came. Johnny's comrades were kind and wrote as much about it as they could. The letters were consoling, helpful, yes, hopeful. Perhaps he was safe: Sure, Johnny was safe, that's right. Weren't the wheels dropped to signify a landing? Nobody saw them bail out or crash.

We all prayed and hoped. Then, late one night, the telephone rang. John's mother heard, "Do you know Cracker, and is your son John a flier?" "Yes," she said, "what is it, have you news of John?" The voice answered, "This is a neighbor of yours and I just picked up a short-wave radio message from Germany. It was sent from Stalag Luft No. 1, your son John's name and address were given, and the message read: "AM PRISONER OF WAR AM WELL AND HAPPY DON'T WORRY LOVE TO YOU AND GRANDPA AND GRANDMA HOW IS CRACKER?"

John's mother knew it was real, for Cracker is John's dog! We all rejoiced and cried, too. Prayers are answered, for God didn't forget the devotion of this mother.

Johnny will be with us soon now. And we can hear him say, with his many comrades:

> Peace and rest at length have come
> All the day's long toil is past,
> And each heart is whispering, "Home,
> Home at last."

FRED M. GARLAND.
Assistant Director, Valley View Observatory, Pittsburgh. Pa., July, 1945.

Are Nebulae Gaseous?

It was formerly customary to include in the designation "nebulae" both those objects now known to be "exterior galaxies" of stars and also those enormous luminous or occulting masses of matter, other than stars, lying within the confines of our own galaxy. Current practice tends to limit the term to the latter

objects, and it is these alone that we shall discuss.

Nebulae, so defined, are commonly described as being "gaseous." Gas is defined as "An aeriform fluid, having neither independent shape nor volume, but tending to expand indefinitely."[1] It is questionable whether nebulae are gaseous in the sense of this definition. The question arises as a consequence of the fact that these masses of matter are almost certainly at a temperature approaching absolute zero. The tendency of a gas to expand indefinitely is, according to the generally accepted kinetic theory of gases, proportionate to the absolute temperature of the gas. Molecules of the gas are believed to be in rapid motion in random directions, and the velocity of this movement is a function of the absolute temperature. The moving molecules collide incessantly. Upon collision, they behave like perfectly elastic bodies. The net result of the innumerable collisions is, when the gas is confined, a pressure on the walls of the containing vessel. If the gas is unconfined, this pressure is expressed in the tendency noted, to indefinite expansion of the fluid.

If the molecules of matter in a nebula are at or near the absolute-zero temperature, it follows that they will have little or none of that sort of motion which is necessary to qualify the aggregate as a "gas." There would be little or no tendency for the mass to expand indefinitely. The molecules would be static, except for such random movements as may be purely mechanical, gravitational, or the result of the forces of cohesion. It appears, then, that it is inaccurate to describe a nebula as a "gaseous mass."

In all matter, there exists a mysterious force of cohesion. This force tends to cause molecules to cling together. It is seen in successful operation in all solids, where, in the case of hard metals, the cohesive force is very great. In liquids, the force of cohesion is partially offset by the elastic rebounding of the moving molecules, and in true gases, the cohesive force is quite overcome by the great forces of acceleration imparted to the molecules at each collision, due to their rapid motion.

In a nebula, as we have seen, we may not assume rapid motion of the molecules, except fortuitously. It would appear, then, that, in a nebula, the molecules would tend to remain in whatever position they are placed at "the beginning," except for such random motion as they may possess quite apart from their "gaseous" nature. If two such molecules happened, by reason of their random motion, to approach each other closely, the cohesive force (which we have no reason to believe would not be operating at the extremely low temperature) would cause them to adhere. This increased mass would set up a gravitational field more intense than the uniform gravitational field of the formerly uniformly distributed molecules and would attract neighboring molecules to the aggregate. Since many of the attracted molecules would have little or no motion of their own, this attraction would cause many of them, not to describe orbits about the aggregate, but to "fall" directly into it, increasing its mass and, like a snowball rolling down hill, rapidly increasing the rate of accretion to the original aggregate. Given time enough, the aggregate might grow to a mass equal to that of a star; it might, indeed, become a star. It may be that the stars we see imbedded in nebulae have been formed in this way. It may be even possible that, in some such way, all the stars have been formed from some primordial, random cloud of molecules.

At first sight, it might appear that a process like that described would have, long ago, consolidated all nebulae into stars. But to make this criticism is to

lose sight of the extreme tenuity of the mass of primordial molecules. Gravitational attraction, even of a large mass, would impart almost inappreciable accelerations to remote molecules, and ages would be required to bring into one mass the necessary number of molecules to form a star. On the other hand, if nebulae are true gases, indefinitely expanding, as seems so improbable, space would have been, long ago, filled with a nearly uniform gas of more or less constant—and very low—density.

REFERENCE

1 *Webster's Collegiate Dictionary*, Fifth Edition, 1939.

PAUL E. WYLIE.

2319 St. George Street, Los Angeles 27, California, June 18, 1945.

General Notes

Dr. S. Chandrasekhar, of the Yerkes Observatory, has been elected to membership in the American Philosophical Society.

Mr. Sterling Bunch, an outstanding amateur astronomer of Fort Worth, Texas, died on July 13, 1945, at the age of 45. He was one of the original "Texas Observers" and always was an inspiration to his associates.

Mr. H. J. Colliau, since 1907 instrument maker and superintendent of instrument construction at the Observatory of the University of Michigan, died suddenly at his home on the morning of September 9, 1945. Mr. Colliau was 66 years of age.

Mr. Clyde W. Tombaugh of the Lowell Observatory and the Arizona State College at Flagstaff has been appointed Visiting Assistant Professor of Astronomy in the University of California, Los Angeles, for the academic year 1945-46, to replace Dr. Samuel Herrick, who is on sabbatical leave for the current year. Professor Tombaugh will take up residence in Los Angeles when the fall term opens on October 26, 1945.

Dr. Edward A. Milne, Rouse Ball professor of astronomy at the University of Oxford, who was awarded the Catherine Wolfe Bruce Gold Medal of the Astronomical Society of the Pacific for the year 1945 in recognition of "distinguished services to astronomy," was formally presented with the medal at a meeting of the Royal Astronomical Society in London on May 11. The presentation ceremony was conducted by Sir H. Spencer Jones, F.R.S., Astronomer Royal. —(*Science*, June 22, 1945.)

New Director of the Hayden Planetarium

The appointment of Lieutenant Commander Gordon A. Atwater as Chairman and Curator of the Department of Astronomy and the Hayden Planetarium was made known recently by Mr. A. Perry Osborn, Acting President of the American Museum of Natural History. Commander Atwater has been released from active duty with the U. S. Navy as of September 1 when he assumed the important position of directing the Planetarium, its public performances, and its program of instruction in celestial navigation for the armed forces.

As Head of the Navigation Department of the Naval Training School Ad-

vanced at Fort Schuyler, Commander Atwater worked closely with Professor William H. Barton, the late Chairman of the Planetarium, and with Miss Marian Lockwood, Acting Chairman since Professor Barton's death on July 7, 1944.

Commander Atwater's intense interest in astronomy and navigation are of long standing, dating back to his boyhood when he and his brother built and navigated their own boat on Lake Erie at Cleveland, Ohio. His appointment to the top-ranking post in the Hayden Planetarium climaxes a lifetime study of applied astronomy.

Commander Atwater's proposed activities for the Planetarium in the near future include expansion of navigation training and piloting classes for special groups; continued cooperation with the Navy, Coast Guard, Merchant Marine, and Army navigation schools with emphasis on navigational techniques and the development of navigation as a science. Popularization of the public lectures in navigation and astronomy will be continued as will the close association with the New York City school system. Further cooperation with hobby groups such of Power Squadron classes, yacht clubs, and amateur astronomical organizations are planned. Exchange of scientific knowledge will be stimulated by inviting scientists and educators from other institutions to cooperate actively with the Planetarium and by Planetarium staff members giving invitation lectures on special topics elsewhere.

His understanding of the broad scope of the Hayden Planetarium and its relationship to the city and nation it serves makes Commander Atwater uniquely qualified to take over the leadership of this large educational institution.

Dr. Harold L. Alden succeeds Dr. S. A. Mitchell as Director of Leander McCormick Observatory

At the meeting of the Board of Visitors of the University of Virginia, Dr. Harold L. Alden has been appointed Professor of Astronomy and director of the Leander McCormick Observatory.

In order that Dr. S. A. Mitchell may continue his official connection with the University and may continue to carry on his astronomical researches at the observatory, the Board has appointed him "director emeritus." Dr. Alexander N. Vyssotsky has been promoted to the rank of Professor.

Professor and Mrs. S. A. Mitchell have moved from their home on Observatory Mountain which they have occupied for 32 years, since 1913, and are now located in their new home at 1419 Hilltop Road. The home of the director, Observatory House, will be the residence of Dr. and Mrs. Alden. In returning to live in Charlottesville, they are coming back home after an absence of 20 years.

Dr. Alden was born in Chicago. After receiving his bachelor's degree from Wheaton College (Ill.) in 1912 and his master's degree from the University of Chicago in 1913, he came to the University of Virginia in 1914 and obtained his Ph.D. degree in 1917.

In the year preceding Alden's arrival, Dr. Mitchell had procured a photographic attachment for the 26-inch refractor. In July, 1914, Dr. Charles P. Olivier was appointed adjunct Professor, and immediately Mitchell, Olivier, and Alden started a most active career in photographic astronomy. In fact, the McCormick Observatory soon became known to astronomers as having one of the busiest telescopes in the whole world. Up to the present more than 55,000 photo-

graphs have been taken with the great telescope.

In 1925, Yale University had constructed a new telescope of the same aperture, 26 inches, as the McCormick visual refractor. The Yale instrument, a photographic refractor, was sent to Johannesburg, South Africa, to photograph the stars in the Southern Hemisphere that could not be reached by the northern telescopes. Dr. Alden has been in charge of the Yale telescope since the date when it was put into operation by the then Prince of Wales and until the present. In his 20 years in Johannesburg, Alden participated in and supervised the taking of 200,000 exposures with the 26-inch photographic refractor. The plates were for the determination of the trigonometric parallaxes, proper motions, mass-ratios, and orbital motions of stars; positions and motions of the satellites of Jupiter, Saturn, and Neptune, positions of minor planets, etc. In addition, several thousand exposures were made with a wide-angle camera for the rapid cataloging of star positions and for the determination of the errors of star places.

During his 20 years in South Africa, Alden had many honors conferred on him. He served as a member of the South African National Committee of the International Astronomical Union and as a member of Commissions 24 and 32 of that Union. He was committee member, vice-president and president of the section on physical sciences of the South African Association for the Advancement of Science, and was also council member, vice-president, and president of the Astronomical Society of South Africa.

In the important astronomical research of measuring stellar distances, the Yale telescope, the Allegheny photographic refractor, and the McCormick Observatory have been the leaders in determining stellar parallaxes. With its grand total of nearly 1800 parallaxes the McCormick Observatory is slightly in the lead.

With his 11 years experience in Virginia and 20 years in South Africa, Professor Alden in taking over the directorship of the Leander McCormck Observatory is recognized as one of the foremost authorities on parallaxes in the whole world.

American Astronomical Society

The annual business meeting of the American Astronomical Society was held at the Harvard College Observatory, June 8, 1945, at 8:00 P.M. It was preceded by a dinner meeting of the Council at the Harvard Faculty Club. Total attendance was 39 members, practically all of whom were from the Boston area.

The following officers were elected:

Vice-President	1945-1947	Alfred H. Joy
Secretary	1945-1946	Dean B. McLaughlin
Treasurer	1945-1946	Keivin Burns
Councilors	1945-1948	N. T. Bobrovnikoff
		A. D. Maxwell
		R. M. Petrie

A nominating committee, whose task it will be to prepare the list of candidates for the election in 1946, was elected as follows:

Joel Stebbins, Chairman
B. J. Bok
Dirk Brouwer

No decision could be made as to the time and place of the next meeting, owing to government restrictions on conventions.

The Henry Norris Russell Lectureship

At the business meeting of the American Astronomical Society on June 8, 1945, the Council approved the establishment of the Henry Norris Russell Lectureship and appointed a committee to raise the supporting funds. This lecture on astronomy or a related field of physical science is intended to be a regular (probably annual) feature of meetings of the Society. It is to be of a technical and scholarly nature and is expected to be an original contribution to science. The lecturer who may be an American or a foreign scientist is to be selected several months in advance by the Council of the Society.

Dr. Henry Norris Russell, Professor of Astronomy and Dierctor of the Observatory of Princeton University, has been a member of the Princeton faculty for 40 years. Through the years, Dr. Russell has carried on numerous researches at the Mount Wilson Observatory also. He is past president of the American Association for the Advancement of Science, of the American Philosophical Society, and of the American Astronomical Society. He has long been regarded as the dean of American astronomers and his influence has been far-reaching in astronomy and related fields in physics. For some time the question of how to honor him in some lasting way had been discussed among astronomers, until the establishment of this permanent lectureship in his name was proposed and adopted by the American Astronomical Society.

The committee for the soliciting of funds is composed as follows: Harlow Shapley, chairman, Walter S. Adams, Joseph C. Boyce, Theodore Dunham, Jr., Luther P. Eisenhart, Charles A. Federer, Jr., Alfred H. Joy, Dean B. McLaughlin, W. F. Meggers, Charlotte Moore Sitterly, Percy Wiherell. Contributions may be made payable to the American Astronomical Society and addressed to the undersigned.

DEAN B. MCLAUGHLIN, *Secretary*,
Harvard College Observatory,
Cambridge, Massachusetts.

Notes on Lunar Craters

The conflict over the origin of lunar craters still simmers with the trend being toward the meteor school who claim that the objects were formed by the impact of large meteors. The volcanic school objects and points to the central cone in many craters as unmistakable proof of their volcanic birth.

Recently Willy Ley made some experiments of dropping objects into powder and was able to form central peaks in his man-made craters. He suggested that people who had done this previously were not careful enough about the experimental materials used and so failed to get the central cones.

Recently it has been the pleasure of this writer to shov the moon thru his 10-inch telescope to a considerable number of navigators and pilots returned from combat and they all stated that the lunar craters "looked like the Anzio beachhead when we got thru with it." I objected on the basis that bomb craters never had a central cone. Many of them then objected, saying that bombs often made craters with central cones. Several of the more observant went on to point out that the presence of this cone depended on the type of bomb and the type of soil it entered. Also they agreed that bombs which buried themselves deeply before exploding usually left a central peak in almost any soil.

The stories of these men were consistent and these items may add more weight to the meteor school.

Selman Field. WALTER HOUSTON.

Book Reviews

An Easy Outline of Astronomy, by M. Davidson. (C. A. Watts and Co. Ltd. London, E.C.4, England. 2/6 net.)

This small-sized volume of a few more than one hundred pages contains a fund of fact and information quite out of proportion to its size. As stated in the preface, the book is intended for those "who have no knowledge of astronomy." The author, therefore, has included only the simplest ideas involved in this science. He has done this in a way that makes the story a fascinating one. The reader having the preparation afforded by this will be able to undertake the reading of more elaborate treatises successfully. To test the reader's comprehension, the author has inserted a series of questions at the end of each chapter, the answers to which are found at the end of the book. This is a very helpful device.

The book is one in the series called the Thinker's Library, and the standing of the author in astronomical work assures its accuracy and authenticity.

C.H.G.

Dynamic Meteorology, by J. Hölmboe, G. E. Forsythe, and W. Gustin. (John Wiley & Sons, Inc., New York, 1945, 378 pp. $4.50.)

The authors of this book, members of the instructing staff of the Department of Meteorology at the University of California at Los Angeles, have made a worthwhile contribution to the rapidly-growing body of meterological literature published on this continent.

On glancing through the Table of Contents and running rapidly through the book itself, one is struck by two features. In the first place, vector analysis is used extensively and is developed from first principles as the need arises. Secondly, radiation is nowhere discussed. In this latter regard the authors are following continental European practice. (In Koschmieder's "Dynamische Meteorologie" radiation is omitted completely; Exner, in his classic work of the same title, devotes brief section only to radiation.) This omission is hard to understand. The differential heating of the earth by solar radiation is the fundamental cause of atmospheric motions; with no external source of energy the earth's atmosphere would be motionless, in complete statical equilibrium, and there would be no weather. Since the aim of the book, as stated in the Preface, is to give the theoretical background necessary to understand the physical processes taking place in the atmosphere, the omission is unfortunate. The only explanation given (p. 27) is that radative processes act relatively slowly, and are therefore not considered, but that is hardly adequate.

The treatment falls into three main sections, the first on thermodynamics, the second on statics, and the third on hydrodynamics. The first group (Chap. 1 to 3) includes discussions of dimensions and units, thermodynamics of a perfect gas, and the thermal properties of water in its several phases and of moist air. The second group (Claps. 4 and 5) includes hydrostatic equilibrium and stability. The last and most extensive grouping (Chaps. 6 to 12) covers the equation of motion, horizontal flow, the wind variation with height in the surface layers and in the free atmosphere, the mechanism of pressure changes, circulation and vorticity, and waves in a zonal current.

There are a number of commendable features in the authors' treatment. It is refreshing to have, in a meteorological text, a high degree of precision in the definition of terms. When developing a new topic, the authors are scrupulously careful to give an adequate preliminary discussion. For example, see the first several pages of Chaps. 4 and 11. As a result, the reader, given a good foundation, is able to follow the later discussion more readily than if he is left in the dark as to the precise meaning of terms used. The treatment of thermodynamic charts in general is especially good, the best available, in the reviewer's opinion.

One is impressed by the thoroughness with which certain of the hydrodynamical concepts are developed. Thus in Chap. 11 the circulation theorems are given for absolute motion in their primitive form, and then in developed form. The former give the instantaneous circulation around arbitrary closed curves, and the latter give it for curves representing the same individual fluid particles at all times. Finally the formula for individual circulation changes relative to the earth is derived. After having followed through these developments, the student should have a good grasp of the significance of the various theorems, and know where they can, and where they cannot be applied.

On the other hand, there are features which are open to criticism. Are the advantages of the meter-ton-second system great enough to warrant its adoption in preference to the well-established centimeter-gram-second system? Although definitions are generally good, there are sveral which depart from accepted usage. Thus, a conservative property is not one which remains invariant under *dry-adiabatic* processes only, as suggested on p. 31. It is regrettable that the authors have decided to minimize the use of entropy in their treatment, because of its abstract definition, and the fact that it may be bewildering to the student (p. 34). Entropy is a fundamental thermodynamical variable, and its use would have simplified later derivations, such as that of the adiabatic relationship for saturated air. In the treatment and use of the tephigram (p. 39 and later) the advantage of rotating the chart is not obvious. To those who have used the chart for years the vertical isotherms and horizontal dry adiabats constitute one of its main advantages. Another merit of the tephigram not mention in the book is the large angle between dry and saturated adiabats.

The treatment of water in its various states is thorough, perhaps unnecessarily so from the point of view of the meteorologist. Normand's classical treatment of the wet bulb temperature is not given, and the adiabatic wet bulb temperature is not defined in physical terms (p. 77). As a result, the authors' treatment does not bring out clearly the important fact that the wet bulb temperature follows the saturated adiabat for adiabatic motion. There is confusion in the definition of latent instability. It is surprising that potential (convective) instability is not mentioned, for it is a concept which appears widely in the literature.

From p. 141 to p. 144 there is a discussion of rate of precipitation. The weather forecaster, if he is to do good work, must keep a number of different factors in mind, and fit them together into a mutually consistent and physically possible whole. The example given on p. 144 is not a good one. It supposes a vertical velocity of 30 cm per sec., a value probably only attained in clouds of the cumulus type, or in special phenomena such as hurricanes or tornadoes. The initial temperature of the isothermal layer is 10° C., yet according to Bergeron's widely accepted hypothesis, precipitation heavier than drizzle does not occur unless part of the cloud at least is at sub-freezing temperatures. Nevertheless, the authors find that a downpour of 6.6 mm. per hour occurs.

Chap. 7, on Horizontal Flow, seems to give an unnecessarily lengthy and complicated devolpment to arrive at the geostrophic and gradient wind relationships. The omission of the isallobaric wind component is surprising, for it, in spite of its shortcomings, gives, in conjunction with the geostrophic wind, our best approximation to the true wind. Chap. 9 discusses the effect of turbulence on the wind in the surface layers. In discussing the analogy between molecular and eddy motion (pp. 248 and 249) mention should be made of the fact that this analogy only serves as a first approximation, and that discrepancies noted in other applications prove that it must be used with caution.

Few misprints have been noted. The superior dot denoting total differentiation with respect to time is missing in several places (pp. 186, 187, 199, and 223). Other symbols are sometimes missing. The book is written in very good English, with a straightforward style. Diagrams are plentiful, clear, and often ingenious. There are a few exercises worked out, but no problems provided.

The reviewer has had the feeling on reading this book and some others produced in wartime, that the development shows the effect of those special conditions. Students often do not have the background in mathematics and physics which would be required in peacetime, and teachers must perforce provide at least part of that background. Thus we find books produced which assume less formal training than those published earlier under normal conditions. For this reason, the book under review will be read with profit not only by prospective meteorologists, but also by practicing meteorologists who may have forgotten some of their mathematics and physics, but who nevertheless wish to keep up with the rapidly developing theory in their field.

E. Wendell Hewson.

Meteorological Service of Canada.

Practical Marine Navigation, by James A. Stowell. (Addison-Wesley Press Inc., Cambridge 42, Mass. $2.50.)

This volume consists of 133 lithographed pages of discussion and six pages of index. The style, purpose, and scope of this work are set forth by the author in the introduction, as follows:

"The volume has been compiled in a systematic form, starting from the very beginning of the science, with the terms and definitions employed and the compass, and advances step by step into the problems, treating each separately, and combining these parts into the practical everyday problem which the navigator will use in his work. . . It has been my foremost and constant thought in compiling this book to make it brief, easy to read and understand, and to make every word, phrase, and example of practical value."

The first 63 pages are used to discuss methods of geo-navigation, that is methods using only terrestrial reference points. The next 60 pages explain methods of celo-navigation, as the author calls it, that is navigation which makes use of celestial objects for reference. The latter requires the introduction of certain basic astronomical concepts which is done. There are numerous illustrations in the text and also many examples completely solved. The several classical methods are explained and illustrated.

C.H.G.